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## PART I.

## ORIGINAL COMMUNICATIONS.

ART. VII.—*Remarks on the Semeiological Value of the Pulse in Childbed.* By ALFRED H. M'CLINTOCK M. D., F.R.C.S., President of the Dublin Obstetrical Society, Hon. Fellow of the London Obstetric Society, Vice-President of the Dublin Pathological Society, Master of the Lying-in Hospital, Dublin, &c.[a]

THIS paper does not by any means pretend to be an essay on all the changes and modifications which the pulse undergoes in the physiological and pathological states of childbed: its object merely is to point out some cases which form exceptions to the general rules bearing on the interpretation of the pulse of puerperal patients.

These general rules, briefly stated, are, 1st, that where the frequency of the circulation does not exceed 80 in the minute, the patient may be considered as going on favourably; and, 2nd. that a pulse at 100, or upwards, indicates the presence of danger, or at all events of some mischief.

The careful observation of the condition of the pulse is

[a] Read before the Association of the King and Queen's College of Physicians, March 6, 1861.

a duty of primary importance in the case of the childbed woman, and never should be omitted. Indeed, I venture to affirm that of all the symptoms belonging to the puerpera, taken singly, the pulse is the most valuable and the most reliable. Nevertheless, even the pulse may deceive the practitioner; and the present communication is intended to point out a few of the occasions where extreme circumspection is necessary before forming an opinion from the character of the pulse.

Of the different characters of the pulse as to *rythm*, *force*, and *frequency*, I have been accustomed to attach most importance to this last, viz. the rate of frequency. The other two, however, will never be lost sight of by the practitioner, as the force or strength of the pulse will often decide serious questions of treatment. Frequency alone will sometimes do the same thing; for a pulse above 120 or 130, no matter what its other characters may be, generally suggests extreme caution in the employment of depletory measures,—as much so, indeed, as a weaker pulse at 100 or 110. The late Dr. Todd has given the opinion—and all practical physicians must concur in it—that there is no one symptom so important as the pulse. But in his estimation, it is not so much the frequency, as the volume, the quality of the pulse, which bears a direct ratio to the vital powers of the patient. From the context it would seem as though he intended this observation to apply to cases of fever only; and if so, I believe his proposition will be generally admitted. To ascertain the frequency and rythm of the pulse is a simple matter, and can always be absolutely determined; but to form a correct opinion of the qualities comprehended under the term force or strength, which includes volume, compressibility, &c., requires great experience, and a very delicate tact.

The rules ordinarily laid down for taking the pulse—such as not doing so till the excitement caused by our first saluting the patient has subsided, and not reckoning the pulse for a shorter period than a quarter of a minute—apply with even more cogency in the case of the puerperal patient.

In a large proportion of the cases of normal convalescence from parturition, the pulse scarcely deviates at all from the standard of health. With the reaction succeeding to the shock of parturition, and with the coming of the milk, there may be a rise of a few beats; but even these temporary accelerations are more frequently absent than present. I continually meet with cases where the pulse does not exceed 72 all through childbed; and many other cases I have met with where it was as low as 60, 54, 48, and even 44. This morbid slowness was constitutional, I believe, and did not depend on any condition

peculiar to childbed. The late Dr Montgomery met with some cases of this kind; and my observation entirely agrees with his, that these women recover well. In none of them could I find any cause for the remarkable torpor of the circulation[a].

If the patient's pulse be 80 or 84, she is considered to be going on favourably; and, as a general rule, this inference would be correct. Let me point out, however, some cases where this reasoning would be fallacious, and where, though the pulse is quiet, considerable danger may be present, or, at all events, the patient's position may not be at all so satisfactory as the rate of the pulse would seem to indicate.

It has been thought by Beck, Hardy, and other observers, that where ergot has been given to the extent of two or three drachms, it may be followed by a depression in the rate of the pulse, lasting for two or three days, which might mislead us as to the true condition of the patient. The diminution so produced, however, is observable on rare occasions only, and seldom, I believe, amounts to more than ten or twelve beats in the minute; but this much would make all the difference between a normal and abnormal rate of frequency—that is to say, a pulse which but for the ergot would have been 96, may be only 84 in the minute.

I have frequently had occasion to observe—and no doubt so have many other obstetricians,—that in the commencement of metritis, there is oftentimes a definite period during which the vascular system shows no obvious sympathy with the local influence—the pulse not ranging above the ordinary standard. This tranquillity of the circulation does not last long; a few hours will find the patient in a state of general vascular excitement, with a hot skin, and a pulse at 96 or 100.

Here the local symptoms of inflammation are apparent for some hours before the pulse gives any unequivocal indications of the mischief that is going on, and hence the importance, I may incidentally remark, of always examining the condition of the uterus in the puerperal patient. With regard to the order of sequence of rigor, local pain or tenderness, and vascular disturbance, in cases of uterine or peritoneal inflammation, considerable differences are observable. The rigor may be al-

[a] The most remarkable example I have seen of depression of the pulse was in a patient delivered of triplets. She was a thin delicate woman, and had come to the full time; the pains were energetic, and her labour most rapid; and such was the degree of collapse succeeding to it, that the pulse fell to 30, and I scarcely thought she would have survived: yet she recovered, however, and bore children subsequently. This woman had not naturally a slow pulse.

together wanting, and, as a general rule, these are the least formidable cases; or it may precede the other two symptoms, or it may follow them; or, again, disturbance of the circulation may take the initiative, and in the course of some hours, we find local symptoms of inflammation to make their appearance.

From paying too exclusive attention to the pulse, and under-estimating local symptoms, the attendant might be led into a culpable inactivity of treatment, and thus allow the disease to gain considerably ahead of him before employing sufficiently energetic remedial measures; and this error I myself more than once committed, at the outset of practice.

In cases such as I have been describing, although the rate of the pulse may not point out the presence of any lesion, yet the educated finger will seldom fail to observe a sharpness or quickness in its beats which is not the accompaniment of a healthy pulse.

Uterine tenderness may, no doubt, be due to other causes besides inflammation; but where the practitioner has any misgiving on this point, he should see the patient at short intervals, and thus, by vigilantly watching the course of events, adapt his treatment to the nature and exigency of the case.

A rigor may come on at any time in the course of an attack of puerperal fever (though it more commonly is one of the initiatory symptoms); and it not unfrequently happens that for some time,—I cannot say for how long exactly, but I think only an hour or two—previously to the fit of shivering, there is a marked diminution in the rapidity of the circulation, leading the attendant, if he should happen to see the patient at this period, to believe that the disease is yielding, when in truth it is gaining strength, and, it may be, getting beyond the reach of art. This pleasing delusion is soon dispelled, to the intense disappointment of friends, and the great mortification of the doctor, who has to recall the sanguine opinion so recently expressed, and to substitute one of grave foreboding. A knowledge of the fact just stated, obviously suggests the necessity of caution in drawing a prognosis from the character of the pulse at one visit. This precept is strictly in accordance with a maxim well known to practical accoucheurs in regard to puerperal fever, namely, not to regard any amendment in the patient's condition as real and genuine, unless it continue for twenty-four hours. The reason for this precept is, that a remission in the symptoms is apt to occur in the course of this disease, and may deceive even the most experienced practitioner, if he judges from the present condition of the patient, and without waiting for the test of time.

As the pulse may be lowered by an approaching rigor, so it may be reduced in frequency by the presence of nausea. This may be accidentally induced by medicine, or be a symptom of the puerperal disease; in either case, the nausea may depress the pulse, and should therefore be taken into account, when estimating its semeiological importance.

In the progress of cases of pyæmia, rapidity of pulse is a constant, indeed, almost an unfailing, attendant; yet even in those running on to a fatal issue, I have sometimes seen the pulse temporarily fall so low as 70 or 80,—not on the approach of a rigor, but after the sweating stage succeeding to rigor, and apparently the effect of extreme exhaustion. I need do no more than barely allude to the possibility of the circulation being also lowered in such cases by the action of some medicine administered to the patient; opium in large doses, or digitalis, are those most likely to have this effect. Of the *veratrum viride*, so highly extolled for its sedative effect by American writers, I have no experience.

Having thus mentioned some of the qualifications with which we are to receive the maxim that "a pulse at the normal rate is evidence of a safe or favourable condition of the patient," let me now take the other side of the question, and point out some of the exceptions to the rule that "rapidity of the circulation is an indication of danger." And I may here observe, that a slow pulse more surely indicates a healthy state of things, than does a rapid pulse the reverse; in other words, the association of a slow pulse with puerperal disease is more rare than that of a rapid pulse with health, so numerous are the causes which tend to disturb the circulation at this time.

This division of the subject is much more extensive than the one I have just been engaged in considering, and the following remarks are to be regarded merely as a small contribution towards its elucidation.

Although the physiological state of the circulation during childbed is not one of excitement, nevertheless it is an eminently *excitable state*. That extraordinary susceptibility of the nervous system, which constitutes so prominent a feature of pregnancy, reaches its culminating point during this period, and furnishes at once a satisfactory explanation of many of the accelerated changes of the pulse, and teaches an important lesson in the hygienic and moral management of this class of patients.

We shall find it convenient to classify the cases of which I am about to speak under three heads, according as the pulse is *momentarily*, *temporarily*, or *permanently* quickened, a clas-

sification not only useful for purposes of arrangement, but of the highest practical value.

The causes which are capable of producing a *transient* excitement of the pulse in the puerperal patient are literally innumerable—a much more trivial thing acting as an excitant now, than would be sufficient at any other time. Any slight mental emotion or bodily disturbance will have this effect. Some altercation with the nurse, some muscular effort, or the entrance of the doctor into the room, are among the most common causes. Scarcely a day passes that I have not occasion to observe this momentary quickness of pulse produced among my hospital patients, by the simple circumstance of their asking for a pass to admit a visitor. In many newly-confined women, the act of nursing, even though unattended with any pain, and performed in a perfectly easy recumbent posture, is accompanied by a rise of ten or twelve beats in the pulse. This I have repeatedly noticed. It may be safely affirmed that an acceleration of the pulse, which is only of the transitory kind, cannot be of any consequence; and we can generally ascertain, within the limits of an ordinary visit, whether the hurry of the circulation be of the kind we are now considering or not. This I would call the natural or physiological irritability of pulse; but we must remember that there is such a thing as a *morbid irritability* of the circulation where the rise in frequency is disproportionate to the exciting cause. Here, caution and circumspection are necessary; for this condition of the pulse is apt to presage some form of puerperal disease.

The quickness of pulse does not always subside, however, so rapidly, but may continue for some hours. These cases are of more importance, and very justly occasion more uneasiness in the mind of the medical attendant. All such cases I would group together under the head of *temporary* acceleration of the pulse. Although this distinction may appear somewhat refined, yet it is not purely arbitrary; and there is this much also to be said in its favour, that the causes which operate in temporarily disturbing the circulation, are generally of a different kind from those in the class of cases last considered. Foremost among these causes is loss of rest, and the improper use of stimulants.

Nothing tends so powerfully as sleep to restore the equilibrium of the nervous and circulating systems after labour; and where a patient is deprived of her needful repose, the ill effects of this are exhibited in the quickened state of the pulse, the headache, and general nervous irritation.

Where the loss of rest is produced by any cause external to the patient,—such as a cross child, a snoring or coughing

nurse, or by noises in the house, &c.,—it should be removed, so far as is possible; but if its removal be impossible, a very good plan is to lessen the patient's acoustic sensibility, by putting wadding in her ears, a plan I have adopted with great advantage on many occasions. If the sleeplessness depends on the patient herself, we must only try and overcome it by opiates in some form.

A continuance of sleeplessness, with the arterial and nervous excitement consequent thereon, may be productive of serious ill-effects, mental or bodily, to the woman.

That the premature or too liberal employment of diffusible stimuli, in the shape of wine, brandy, or malt liquors, will occasion an over-excited state of the circulating system, is known to every one. If the physician have ordered any of these, he can very easily ascertain whether, or how far, the quickness of pulse is due to their influence. But when he is only cognizant of their effects, and in total ignorance of their administration, then indeed there is some excuse for his perplexity and alarm. Cases of this kind occur now-and-again from the clandestine use of the liquors in question; the patient, or patient and nurse, being to blame in the matter. Now, I cannot lay down any single rule whereby to diagnose this particular cause for excitement of the pulse. Of course, the practitioner would examine the patient with the strictest scrutiny, to see if disease be lurking in any part of the system. A negative result of this examination, together with the flushed countenance, the slightly excited manner, perhaps the odour of the breath, and, lastly, the time of the day when these symptoms are present, may lead to detection, or awaken suspicion. I remember attending a lady some years ago where this deception was very successfully practised upon me; and, no doubt, my daily anxious inquiries, and minute investigations to discover the hidden cause, afforded no little amusement to the patient and nurse, who, in this case, were in league together. It was not for months afterwards I found out the true explanation for the symptom which had so alarmed and perplexed me. Acting under the advice of the nurse, this lady was taking three or four glasses of port wine daily, in addition to some claret.

The nurse, however, is not always an accomplice in the imposition, and discovery in such cases is more easy.

We must not forget there are slight febrile attacks of the kind, commonly called "weid," which commence with a chill, and are frequently connected with some mammary irritation. These are attended with excitement of the circulation, which subsides in the course of 24 or 36 hours.

We come now to the third division of this subject, comprehending those cases in which the morbid frequency of the pulse is, by comparison, *permanent*, that is, remaining constant for some days, at least.

Although a continuing excitement of the pulse within the ten or twelve hours succeeding to delivery is always a symptom that calls for extreme caution and circumspection on the part of the attendant, yet it does not in every instance necessarily indicate the presence of organic disease, or of danger to the patient. In conformity with the plan of this paper, I shall endeavour to sketch very briefly some of the cases in which this symptom presents itself, *independently of any existing puerperal disease.*

When we meet with this excited state of the circulation, it is, no doubt, most satisfactory to find, upon a full and patient investigation, that it is a solitary symptom, unassociated with any other of a morbid kind; this, I say, relieves our mind of a great deal of apprehension. But then we naturally seek some cause for this vascular disturbance, and, till this be discovered, we are only half informed upon the case; and cannot with confidence assert that this symptom is not connected with organic lesion, nor can we venture to prescribe with any well-assured expectation of success.

Immoderate hemorrhage during or after labour may, as on any other occasion, be followed by an excitement or irritability of the pulse, persisting for many days, answering to Marshall Hall's description of "hemorrhage with excessive reaction." I rather think the puerperal condition to be peculiarly favourable to the production of this sequela of hemorrhage.

Of the puerperal patients who have had hemorrhage, all will not subsequently have a rapid pulse. It is much more likely to occur in some women than others; and I have observed that women of a pale, cachectic look, who lead sedentary lives, using little exercise, and rarely going into the open air, are peculiarly liable to this effect of hemorrhage, owing, no doubt, to the greater irritability of habit which this mode of life induces, and also to the deficiency of blood which is usually observed in these patients.

Extreme caution is needed before pronouncing, in any given case, that the frequency of the pulse is solely due to the hemorrhage; for flooding, as Denman has remarked, is a strong predisposing cause of puerperal fever, oftentimes of a low and very insidious form.

The inordinate use of tea for a length of time before labour, would seem to be capable of causing an increased rate of the

circulation. At least, I have now observed this connexion in many instances where nothing else could be assigned for the production of the symptom in question. These patients have generally been workwomen, accustomed to late hours, and with whom tea—often very strong tea—was an accompaniment of every meal, and constituted the staple article of their food.

I remember attending a lady (she was also seen by Dr. Hugh Carmichael), some sixteen or seventeen years ago, who was an exquisite example of this derangement. From day to day, after delivery, I was greatly alarmed to find her pulse ranging from 116 to 130. At length, one day, observing my portentous aspect, and guarded prognosis, she burst out a laughing. "Doctor," she said, "you are frightened at my pulse being so fast; but you needn't, for I can tell you the cause for it." Thereupon she acquainted me, that all through her pregnancy she indulged to an unlimited extent a craving she had for raw tea, which she always carried about with her, and used to eat dry out of her hand.

As I saw none of these patients prior to the setting in of labour, I cannot say what the pulse was then, but should suppose it was somewhat above the natural standard.

Where the act of nursing is productive of much suffering, whether from soreness or morbid sensibility of the nipples, the pulse will be considerably quickened by it; and if the child be frequently applied to the breast (under the mistaken notion of preventing mammary inflammation), time will not be given to allow the pulse to subside between one act of suckling and the next.

Patients labouring under organic disease of the heart form another group of cases where undue frequency of the pulse is occasionally observed after labour. I say "occasionally," for not every patient so affected shows this morbidly increased velocity of the circulation; nevertheless, I have now seen a good many instances of it.

When there exists valvular disease,—and nearly all these patients had signs of valvular disease, in some of its forms,—we can, I think, have little difficulty in understanding how any considerable change in the circulation must affect the action of the heart, in consequence of its power of adjustment or accommodation being so materially impaired. That the act of parturition makes a great change in the distribution of the blood, and general conditions of the circulation, is self-evident; and hence, I suppose, the great and undue excitement of the diseased organ. The incapability of the heart to make the re-

quired adaptation has proved a cause of death on some rare occasions.

The extraordinary influence exerted by the state of the mind over the organic functions of the childbed woman is one of the most striking and important features of the puerperal condition. The circulation participates largely in this influence; and accordingly it is not at all uncommon to find a quickened pulse in patients who are suffering under mental dejection, anxiety, or suspense, particularly if of recent origin. This is so well known, that it would be needless to enlarge upon it. Amongst patients at the hospital, a seemingly causeless quickness of pulse often suggests an inquiry into their social state or domestic circumstances, when we discover, for the first time, some hidden grief, or corroding care, which is the perpetual source of irritation.

An excited state of the pulse often precedes, for some days, an attack of puerperal mania; which is not at all surprising, inasmuch as mania itself is often brought about by mental inquietude of one kind or another. In patients of this description, if to quickness of pulse there be added sleeplessness, loss of appetite, and indifference to the child, we then have a combination of symptoms sufficient to justify the apprehension of a maniacal outbreak, and the adoption of such precautionary measures as prudence would dictate under these circumstance.

A very rapid pulse is on some occasions the first symptom —and for hours the only symptom—of the invasion of scarlatina, a disease most fatal to recently confined women.

In the winter of 1855-6, scarlatina was very prevalent in this city, and a number of the patients in the Lying-in Hospital had the disease after delivery. In several of them there was a very great and apparently unaccountable acceleration of the pulse for twelve, eighteen, or twenty-four hours before the occurrence of the eruption or of the soreness of the throat.

Dr. Churchill has recorded two very striking instances— one of them ending fatally—of extreme rapidity of pulse coming on within forty-eight hours after parturition, and apparently referable to latent scarlatina, certainly not to any puerperal disease, properly so called.

I shall now bring these cursory remarks to a close. I am very well aware that the subject is far from being exhausted, as I have only brought forward those cases which are of more common occurrence ; whilst I have purposely omitted any account of the variations of the pulse induced by puerperal disease.

I will be excused for again urging the necessity of carefully

noting the state of the pulse at every visit to the puerperal patient. The importance of its indications can hardly be overestimated; and where it ranges above the healthy standard, this should be regarded as *primâ facie* evidence that all is not right, and should lead to minute investigation; and even where no puerperal disease can be discovered to account for the symptom, a guarded prognosis should still be given, and cautious treatment adopted.

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## Art. VIII.—*Chronic Subacute Arachnitis.* By H. R. de Ricci, M.D.

We read in Cruveilhier's celebrated work on Human Pathology, the following interesting account:—"I shall ever reproach myself," says this distinguished writer, "for having committed the following serious mistake about a young school-boy aged eleven. He was of an idle disposition, and often pretended to be sick. When called in to see him, I found the aspect of his countenance natural, his pulse normal. He lay doubled up in his bed, without complaining of any pain, but was constantly asking to have stories read out to him. I ordered him up; and he then sat down near the fire, leaning against the sides of the chimney-piece. I made him walk before me, and he reeled like a drunken man. As he would not dress himself, his clothes had to be put on by force; and he even got some slight strokes of a cane; for every one was persuaded that the boy was only malingering. I was, however, very soon made aware of my serious mistake. In a short time the child died! died of subarachnoid meningitis!" This interesting account made a profound impression on me; so that I was ever after particularly cautious, whenever I met with cases where, without apparently any visible cause, the subject of my observations was, contrary to habit, either taciturn, morose, or sulky. I remember once being called to see a person who apparently seemed to be in rude health; her age was about forty-eight years. She did not complain of anything in particular, only said she felt ill. Her aspect was natural, her pulse good, her appetite excellent, and her rest unbroken; but still she said she was ill. On the third day of my attendance, she told me she *thought* she had a pain in her head; and on my asking her to walk across the room, she stood up and put out a hand, as if to hold on by something, or as a person would do walking in the dark. On the fourth day it seemed to me as if her answers, though correct, were slower; and on the following

day, having had the opportunity of speaking to an old friend of hers, I obtained such information as confirmed me in the diagnosis I was gradually arriving at in my own mind. Up to this time my treatment had been of the simplest kind. Having been informed that the catamenia had ceased for about six months, I had ordered six leeches to the perineum, and some mild aperient medicine; but when I now was told that for some time previously the patient had been losing her memory, and that so long as six weeks before she could not at times remember in the evening where she had been to visit in the day, I determined on adopting a more decided treatment, and applied at once twelve leeches more to the perineum, and a blister to the back of the neck. The following day, the fifth, the patient was rather worse; her pulse, though of normal strength, beat only 46 in the minute. When asked how she was, she took some time to consider, and then answered, "Very well;" but I remarked that when she smiled her mouth was slightly drawn to the left side; and occasionally a sudden change—a cloud—would pass across her countenance, and she would carry her hand to her head, laying it on a spot corresponding to about the middle of the left parietal bone. When asked what ailed her at those moments, she would tardily answer, "I do not know;" when it would be suggested to her if it was a pain, she would answer, "Yes." There was no difference in the power of grasping of the two hands; nor did the mouth show any deviation, except in smiling. All these symptoms, so trivial apparently to the uninitiated, filled me with alarm, greatly to the surprise of the patient's friends, who still saw nothing in her to make them apprehensive. I determined, however, if possible, to be in time; and having fully made up my mind that it was an insidious case of subacute arachnitis of the left hemisphere, commenced giving her small doses of calomel at short intervals. On the following day, the sixth of my attendance, all the symptoms were aggravated; the pulse was slower, the mouth more crooked; the heaviness had now become stupor, which, by the following day, passed into absolute coma, accompanied by the unconscious discharge of both rectum and bladder. This was now the seventh day of my attendance; the case had been steadily progressing from bad to worse; and the friends at last, having become fully alive to the correctness of my views, acknowledged with alarm that I had not overrated their relative's danger. What was to be done? The mercury had not as yet produced its specific effect; if such could be obtained soon enough, the patient might yet have a chance, though indeed only a remote one. I im-

mediately ordered the blistered surface on the back of the neck to be dressed with mercurial ointment, and a suppository containing fifteen grains of strong mercurial ointment to be introduced into the rectum. In less than twelve hours she was ptyalized, mainly owing, I think, to the employment of the mercurial suppository, which, after many years of trial, I never knew to fail, whenever I wanted rapidly to produce the specific effect of the metal. At the same time that I adopted this internal treatment, I ordered blisters to the tops of the arms, and sinapisms to the legs and feet, to act as derivatives, and stay, if possible, the mischief going on on the surface of the brain, until the mercury taken up in the circulation could produce its specific effects.

The mischief was stayed, and bravely did the mercurial do its duty; I never saw so clear and satisfactory a proof of cause and effect as in this instance. We had leeched, blistered, and purged; we had applied sinapisms to the legs and feet; but still the case progressed, and the stupor merged into profound coma. Nothing seemed to be of any avail in stopping the onward course of this insidious disease, until the fetor of the breath first, and then the characteristic marks upon the gums, gave unmistakeable evidence of the mercurial action being established; then, and not till then, did the patient rise from that stupor; she opened her eyes, spoke clearly and sensibly, passed no longer under her, and her mouth returned to its normal site. Her pulse, which had been so low, now rose to 70, and soon after to 75,—principally owing, I think, to the irritation caused by the severe ptyalism under which she now laboured. The disease was evidently checked, and every one looked forward with hope to a speedy and favourable termination. The salivation being very abundant, all mercurial applications were stopped; and as the soreness of her mouth prevented her from taking a sufficient quantity of food by that channel, I ordered her nutritive injections, containing each an egg, two ounces of beef tea, one ounce of wine, and three grains of quinia, to be administered every third hour. The blisters on the back of the neck and over the arms were, at the same time, kept in activity by dressing with savin ointment, as I conceived that the continued discharge from them would help to complete what appeared to be the commencement of a cure. Her mouth was, at the same time, freely washed with a weak solution of nitrate of silver, by which means it soon became sufficiently healed to permit her to take food freely. We had now arrived at the fifteenth day of my attendance—I do not say of her illness, as that had evidently been going on for

many weeks before I was called in. On the evening of the fifteenth day, she appeared somewhat heavy and sleepy; she also moaned occasionally, and again her hand went slowly towards the same spot, over the left parietal region. With some difficulty I succeeded in rousing her; and having found that the blisters over the arms were nearly dried up, I applied at once fresh blisters over the same spots, and administered two drops of croton oil, which in a few hours brought away several copious dark-coloured and highly fetid stools, by which she seemed greatly relieved; and on the following morning, at my first visit, I found her sitting up in the bed, eating her breakfast with the greatest relish, without pain, stupidity, or drowsiness. On dressing the fresh blisters, I found a copious discharge; to it, therefore, no doubt, aided by the active purgation, was due the well-marked improvement from the previous evening. Having duly considered this in my mind, I came to the conclusion that the treatment most likely to insure success would be first to establish a permanent purulent drain in the vicinity of the head, and then to continue, if possible, the action of the mercury a little longer, and to follow it up by a course of iodide of potassium, at the same time that the patient was amply supported with nourishment and wine. I therefore told the patient's friends that I considered it necessary to establish two issues, one in the top of each arm, to endeavour to prevent, if possible, the recurrence of a relapse such as had alarmed us so much a few days before, on the drying up of the blistered surfaces. They seemed filled with astonishment at my announcement; they fully believed that the worst was over; that the patient was on the high road to recovery; that I was only an alarmist; and ended by absolutely refusing to permit anything which would constitute, in their eyes, a permanent blemish. I had not been prepared for such a refusal; and thinking that their own good sense would after a few hours prompt them to revoke it, I dropped the subject until the eighteenth day, when I again strongly urged it on them; and on their repeated refusal, I asked for a consultation. The gentleman who was selected to meet me took a most unfavourable view of the whole case, which he regarded as hopeless, although the patient looked better and was livelier than she had been yet since her illness. He agreed with me that the necessity for a permanent drain was clearly pointed out; but he differed in the locality. He recommended the top of the head, in preference to the top of the arms; and deprecating strongly the use of caustic potash, insisted on the issue being established with a blister of two

inches diameter, to be kept open by Albespeyre's paper. I argued strongly for my plan, but in vain; for I knew well the divers idiosyncrasies of my patient, and I knew that she would do anything rather than allow any portion of her head to be shaved. I also felt assured that any cause of annoyance to her should be studiously avoided at this moment, as it would tend seriously to complicate an already too serious disease. I also felt that a blister would never establish the drain I required, at the same time that the locality selected was full of other objections also. However, I argued in vain; with the dull tenacity of age, he persisted in his views, and left the house. As I had anticipated, the lady most decidedly set her face against any of her hair being removed; she insisted that she was better, that she required nothing, and begged to be left alone. This day she had been much better than ever I had seen her during her entire illness; and so I left her in the hands of her friends, who all undertook to persuade her to let the scissors to her head. In about fours I was sent for, to say that she had at last consented to have her head shaved; but that she was rather weak. I went to see her at once, but was shocked at noticing the change that had come on her since the morning; the vacant stare, the flushed cheek, the crooked mouth, the pulse of 130; all told clearly of the mischief that had been done. However, the hair was removed and the blister applied. It never did any good, while the preparation for it did such incalculable harm. On the following morning this poor lady was completely comatose; her pulse had fallen to 96; the buccinators flapped with each expiration, and the rectum and bladder were discharged without her knowledge—fearful contrast with the hopeful condition of the preceding day. Choice would now have suggested to me to abandon the case—duty called on me to remain; I did so, and at once set to work to endeavour, if possible, to allay the irritation that had been freshly lit up in a previously diseased brain by that mental torture which had been inflicted on her for four hours on the previous day. Her stomach was too irritable to take food, and she had some hiccup. I had, therefore, to rest satisfied with ordering her some iced brandy and soda-water, and to return to the nutritive injections. On the following day her pulse was lower, and her face less flushed; but she still lay on her back, unconscious of all that passed around her. The same treatment was continued, with the addition of mercurial dressings to the blisters, which still were discharging. The next morning a fresh symptom had set in; her right arm was stiff; and when I attempted to straighten it, the tendon of the biceps

at the bend of the elbow felt like a cord under the touch. This clearly indicated increasing mischief, and determined me on again making a bold push with the mercury as the only chance, though but a forlorn one, of restoring her to health, at the same time that I established two issues with potassa fusa on the top of each arm. Fearful, however, of making her mouth too sore, and thus stopping up the main ingress to nutrition, I proceeded cautiously with inunctions until the faintest fetor could be discovered from her mouth, when I had her immediately cleansed from all mercurial dressings. The rigidity of the left arm, which for the last three days had gone on increasing, now began to give way; and on the twelfth day from the return of the coma, and the thirty-third of my attendance, it was completely relaxed, and some power was returning in the fingers; the issues, also, were doing well. She still, however, remained in a state of partial stupor; her eyes would occasionally open, but she never spoke; and her stools passed away unconsciously. There was, however, a certain degree of improvement; her breathing was no longer stertorous; she had no hiccup; and she took her food well. Her bowels were torpid; but they could always be roused with small doses of croton oil. She remained in this way till the eighteenth day since she fell into her last state of coma, when, the issues I had put into her arms on the seventh day being in an active state of suppuration, and aided no doubt by the mercury, which had now produced a decided though gentle action on the gums, she opened her eyes and spoke for the first time since the day she became comatose, sixteen days before. On the following day I found her better, and the improvement continued daily, but with one exception—she still passed under her. The power of grasping was equal in both hands; her mouth was perfectly straight, except when smiling; she complained of no pain; eat well, drank well, and slept well; but still the rectum and bladder were discharged unconsciously. Thus she continued for many weeks daily improving, except in that one particular, till on the seventieth day of my attendance she was attacked by what she herself called a *spasm;* she could not describe it, though she was perfectly conscious. It was not a pain anywhere, she said, but it was *all over her*. No doubt, it was some form of epileptoid seizure. On the following day these spasms became more frequent: the day after I remarked that the issues, which for a few days previous had been yielding *less pus* than usual, had become *nearly dry*. I dressed them with cantharides ointment, but to no purpose. On the following day, the right arm became again rigid; the spasms

increased in frequency; the patient became drowsy; she refused her food; and finally, effusion having taken place into the bronchial tubes, she was suffocated on the 111th day of my attendance.

This case, which I have given at such length, was, I think, most interesting, and to me full of instruction. I believe that if I had seen the patient a fortnight sooner, I might have had a different issue to recount. I think that even under the circumstances I attended it, if on the eighteenth day I had been permitted to insert the issues I so strongly urged, she might yet have had a chance of doing well; but that four hours' mental strife was fatal to her recovery; and her brain, already seriously affected, never recovered from the worrying it got on that unlucky day.

I may, perhaps, be asked why I publish a case which ended unsuccessfully. Well, because I believe that we should recount not only our successful cases, but also our failures; for often these last will teach us even better than the former. I knew the power of mercury and the use of issues; but I never saw their value, especially of these last, so clearly exemplified as in this case: they were perfect indicators of the patient's state. When freely discharging, the patient did well; when not discharging, the patient was ill; and when they actually dried up, the last scene closed upon our labours. In the next number of this Journal I shall contrast this case with one of acute arachnitis, which also terminated fatally; but in which I had the advantage of a *post-mortem* examination, which I regret extremely that I could not obtain in this instance.

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Art. IX.—*Plastic Operations on the Female Genito-Urinary Organs*[a]. By Thomas. E. Beatty, M. D., M. R. I. A., F. R. C. S.; Honorary Fellow of the King and Queen's College of Physicians; Ex-Professor of Midwifery, Royal College of Surgeons in Ireland; Physician to the City of Dublin Hospital; President of the Obstetrical Society; Ex-President of the Pathological Society; Corresponding Member of the Obstetrical Society of Edinburgh, &c., &c.

The subject of Plastic operations with metallic sutures has occupied so much attention during the last few years, that I am induced to hope the following cases may not be uninteresting. There is no doubt that the revival of the use of metallic su-

[a] Read before the Surgical Society, March 9, 1861.

tures by Marion Sims was a very great boon to surgery; and although we may not be disposed to go the whole length with him, when he states[a], "I declare it as my honest and heartfelt conviction that the use of silver as a suture is the great surgical achievement of the nineteenth century," we must acknowledge that to him and Dr. Bozeman we are indebted for vast improvements in the treatment of cases that formerly were considered hopeless.

Although, as Dr. Simpson has shown, these very eminent operators have been forestalled by Mr. Gossett, who, in "The Lancet" of November 29th, 1834, describes a case of vesico-vaginal fistula cured by him by means of "gilt-wire suture," and the steps of whose operation bear a very striking resemblance to that of the American surgeons, we must still award the merit of working out and perfecting the process to the latter. It is just as the credit of the discovery of the Atomic theory in chemistry is awarded to Dalton, because he laboured hard, and with great skill and patience completed that theory; which did not originate with him, but was notoriously first promulgated by Higgins, then Professor of Chemistry in the Royal Dublin Society. In noticing the operations for vesico-vaginal fistula, we should not forget that to Mr M. Collis, of this city, we are indebted for the first impetus given to these operations in this country.

His paper on the subject, read before the Obstetrical Society in May, 1856, and published in the "Dublin Quarterly Journal," details an operation differing from those that preceded it, in the proposal to split the edge of the fistula all round, instead of cutting off a portion of it; and then bringing the split edges together by means of quill-sutures. The first operation done by Mr. Collis in this manner, he was kind enough to submit to my inspection, and I found the orifice quite closed. I am inclined to think that this proposal of Mr. Collis to split the edge, instead of paring away tissue, might and ought to be adopted in cases where the opening is very large, and saving of substance is of importance. In most cases the tissue is sufficiently thick to afford of this being easily done; and when it is, there is less strain on the ligatures, of whatever material composed.

It will be seen that, in the following cases, some of the operations differed from that proposed by either Mr. Sims or Bozeman, and partook more of the modifications suggested and practised by Dr. Simpson, to whose lecture, illustrated by wood-

[a] Anniversary Discourse, New York, 1858.

cuts representing the different instruments employed, we are all so much indebted for accurate and useful directions. In one of them, the operation contrived by Dr. Battey, of Georgia, U. S., was for the first, and I believe the only time, performed in this country.

Case I.—M. R. was sent up to me by Dr. Mackesy from Waterford, in the hope that some relief might be obtained from her present lamentable condition. She is a nice interesting person, twenty-six years of age, and two years married. She was confined on the 18th of January, 1859, of her first child. The labour was very severe, lasted twenty-four hours, and was terminated by the assistance of the forceps. During the operation the perineum gave way, and the rent extended through the spincter ani. She was confined to bed for three weeks, and recovered slowly. It is now five weeks since her delivery, and she complains that she cannot retain the contents of the bowels when at all liquid. If they are solid, she can pass them voluntarily but otherwise they escape without her knowledge. Flatus is, in like manner, unconsciously passed. She is still nursing her infant. She entered the private ward of the City of Dublin Hospital, under my care. To ascertain the extent of the laceration, I placed her on her back on a high table, with the legs bent, and the feet resting on the table in the lithotomy position; I then introduced M. Sims' duck-billed speculum into the vagina; and having it well held up towards the pubes, I got a full view of the back of the vagina and the torn perineum. I now found that, in addition to the perineum and anus, the rectum was split up for about an inch and a-half. The edges of the rent were quite healed; and the corners of the lower part, near the verge of the anus, were rounded off, just like the corners of a hare-lip. From the nature and extent of this injury, it was plain that two operations must be performed; the first with a view to close the rent in the rectum and sphincter ani, and the second, at a future time to restore the perineum. As it was of great importance to have the bowels in a quiescent state during the progress of the cure, after the operation, I had them well emptied by medicine, and the rectum well washed out by injections, for three days previously; and the day before the operation, a quarter of a grain of opium was given every fourth hour, to secure torpidity of the intestinal canal. Having made these preparations, I proceeded to operate on the 18th of March, 1859, in presence of Mr. Tufnell, Dr. Churchill, and Mr. Croly, house-surgeon to the hospital. The patient was placed on her

back, as already described, on a high table near a window, with a good light shining into the vagina, now well displayed by the speculum already mentioned, which was well held up by Mr. Croly. The first step in the operation was to pare the edges of the rent in the rectum. This was effected by sticking a hook into the lowest point of the torn sphincter at one side, and with a knife cutting off the edge from the bifurcation above down to the point, just as in the operation for hare-lip. This was repeated on the opposite side, leaving a Λ-shaped cut into the recto-vaginal septum an inch and a-half long. The edges were now brought together by means of iron-wire sutures, passed in the following manner:—A good-sized curved needle, holding in its eye a piece of iron-wire about six inches long, was firmly grasped at a right angle, near to the eye, by a strong porte aiguille, which was kept tightly closed on the needle by a slide run up on the blades. The point of the needle was then struck into the lining membrane of the vagina, close to the upper angle of the wound, on its left side, and at a distance of a quarter of an inch from the cut edge, and driven through the tissue between the rectum and vagina until the point appeared through the cut edge. The needle was urged across the gaping wound until it was made to enter the opposite cut edge at a point corresponding to that from which it emerged in the other; and being forced through the submucous tissue as before, the point was brought out at a quarter of an inch from the edge. When sufficient of the needle was passed through, it was caught by a strong forceps, and the slide on the porte aiguille being retracted, the needle was cast loose on the side where it had first entered, and was pulled out on the right side of the cut, carrying the iron-wire with it. This was now cut off with a pair of scissors near the eye of the needle, and so the first ligature was passed. In a similar way four other iron ligatures were made to traverse the gap in the septum, the last being through the edge of the anus. A thin leaden plate, an inch and a half long, and half an inch broad, with five holes pierced down the centre, was now prepared; and through the holes the ends of the wires were passed, beginning at the upper end, and so on to the last. The shield was then pressed down to the wound, while the ends of the ligatures were held on stretch in the left hand. This had the effect of drawing the cut edges into close apposition; and to secure them in that position, perforated shot were slid down over the ends of each pair of wires; and the shot, seized in a strong pair of forceps, was firmly forced down on the shield; and while the wires were drawn tight, the shot was strongly compressed, so as to hold

the wires securely fixed. This having been done for the five sutures, the ends of the wire were cut off near to the shot, and the ends of the cut wires were bent down on the shield. The operation being thus accomplished, the patient was put into bed, and a quarter of a grain of opium was given every third hour. Directions were given to have the bladder emptied by the catheter three or four times in twenty-four hours, so as to prevent the possibility of the urine coming in contact with the wound. This was strictly attended to by Mr. Croly, who watched the case with the most laudable care. No unfavourable symptom occurred until the 24th, the sixth day after the operation, when a pretty smart hemorrhage took place from the vagina, which lasted only a quarter of an hour, and was stopped by injecting cold water into the passage. On the 26th, the eighth day, the ligatures were removed, in the following manner:—The patient was placed in the same position as on the day of the operation; and the same speculum having been introduced into the vagina, the shield and lead buttons of shot were exposed. With a long pair of sharp-pointed scissors the wires were successively cut across, close to the shield, beginning at the outer one; and all the shot being thus removed the shield came away, leaving a perfectly healed cicatrix, with a surface and edges as smooth as if it had been pressed with a hot iron. An injection of warm water was now thrown into the rectum to soften the fæces, and the bowels were slowly relieved without any laceration of the newly-united parts. This constituted the first part of the operation. It was most successful in its results. The patient regained complete control over the bowels, and nothing escaped without her consent. She remained in hospital for ten days afterwards, when I advised her removal to the country for a short time, previous to any further operative proceeding. On the 27th of April, she returned; and on examination, I found the parts perfectly sound and solid; and so much contraction had taken place, that the gap in the perineum did not appear nearly so large as formerly, and it was quite manifest that a smaller amount of closure would be necessary than was expected. On the 2nd of May, I proceeded to finish the operation. This was done by paring about three-quarters of an inch of the edges of the torn perineum from the anus forwards, and then uniting the cut edges by means of three iron-wire sutures without a button. The ends of the wires were simply twisted together, and cut off about a quarter of an inch from the wound. The same precaution with respect to the urine was observed, as in the former operation. It was drawn off with the catheter every

six hours, and the bowels were kept quiet by opium. On the eighth day the sutures were removed, by clipping one side of the noose with a fine-pointed scissors, and drawing the wire through. The wound was found to be perfectly healed; and the patient left the hospital on the 6th of June, a much happier woman than she had entered it.

Case II.—On the 28th of May, 1859, I was requested by Mr. Banon to see Mrs. H., who had come up to town from Limerick for advice, in consequence of incontinence of urine since her confinement. This had taken place on the 3rd of the month. Her labour was of her first child, twenty-four hours in duration, and very severe. No instruments were used in the delivery. She suffered a great deal of soreness afterwards; and on the sixth day the urine began to come from the vagina, and had continued to do so ever since. On examination, we found a large opening into the bladder from the vagina, of a size sufficient to permit a walnut to pass through; the edges of the opening were thick and fleshy, and in some places granular; the whole mucous membrane of the vagina was inflamed and raw. I expressed an opinion that it was a very favourable case for operation, and at Mr. Banon's desire I undertook the case. On the 31st of the month I proceeded to operate, in presence of Mr. Banon, Dr. Churchill, and Mr. M. Collis. The patient was placed leaning over the edge of a low bed, with her chest supported by pillows, and the buttocks turned towards a window, through which a good light came. The duck-billed speculum of Sims was introduced into the vagina; and being well held up, the aperture in the vagina was brought well into view. The edges were well pared all round. Considerable bleeding took place from the very vascular tissues; this was arrested by tincture of matico; and then five stitches were put in by means of the tubular needle, made by Mr. Young, the eminent cutler in Edinburgh. This needle was first described by Dr. Simpson, and it is figured in his lecture on vesico-vaginal fistula, in the "Medical Times and Gazette" for January, 1859. It is a most perfect instrument, does its work in superior style, and is easily managed. In the operation for vesico-vaginal fistula, the wound being made to close in a transverse direction, the stitches are introduced from before backwards, as the patient lies before us, and the tubular needle enables us to do that with the greatest ease. The wire, cut to the length required, is introduced into the tube, and pushed forward, until the end of the wire appears at the point of the needle; it is then withdrawn, until the end just

disappears within the tube; and having dipped the needle in oil, the point is driven into the membrane lining the vagina, half an inch from the cut surface; and being passed between the bladder and vagina, taking care not to penetrate the former, it is carried out through the raw edge, and then being pushed on, it is made to pierce the far side of the freshly-pared border of the aperture; and being passed as before between the vagina and bladder, it is made to emerge through the wall of the vagina, at half an inch of the other side of the opening. When the point of the needle is seen well above the soft parts, the wire is steadily pushed forwards in the tube, and emerges from the point of the needle. The extremity of the wire is then seized with a long forceps, and pulled well downwards, while the needle is withdrawn over the wire, which it leaves thus safely lodged in its proper position. In this case five stitches were found to be necessary. They were fastened over a leaden shield, or button, differing from Bozeman's in this particular, that while his button has but one row of holes down the middle, through which the wires are brought (two through each hole), the button I used had two rows of holes parallel to each other, or rather five pairs of holes, instead of five single holes, as originally described by Dr. Simpson in the lecture already referred to. The object of this was to get rid of the perforated shot as a means of closing the stitches, and to close the wound by twisting the wires after they were brought through the double holes. The twister originally designed by Dr. Coghill, consisting of an iron rod five inches long, with two very short tubes a quarter of an inch long, attached on either side of its extremity through which the wires were passed, served to secure the stitches. The wires, being passed through the tubes, or rather holes, in the bulb at the extremity of the rod, were held firmly in the left hand; and the instrument, pushed down to the leaden button, was twisted three or four times, making a close and regular cord of the wires. It was gradually withdrawn as the twisting motion was given, and finally withdrawn over the wires; these were cut off within a quarter of an inch of the plate, and the other pairs of wires were treated in the same way, until the whole were secured; the cut ends were then folded down over the plate; a short gum-elastic catheter, with an Indian rubber bag attached to it, was secured in the urethra; the bag had a stop-cock at its other free side, to allow of its being emptied. The patient was placed in bed, lying on her face; a grain of opium was given, and she was ordered a quarter of a grain to be taken every third hour. A good nurse was put

in charge, and Mr. Banon and myself visited her every morning and evening. On our visit the second morning we were startled by hearing that the catheter had slipped out in the night, and that she had passed water by the urethra; no urine came through the vagina. The instrument was now firmly secured, and was removed every day, and washed. No untoward occurrence took place afterwards. On the eighth day I removed the stitches, by placing her in the position as on the day of operation, and displaying the apparatus by means of the same speculum. The stitches were removed by cutting one side of the loop with a sharp-pointed scissors, and drawing out the wire by a forceps. On the removal of the plate, we were gratified to find the whole wound perfectly healed, the cicatrix presenting a uniform smooth surface, as if it had been pressed with a hot smoothing-iron. The catheter was kept in for two days more, the opium was stopped, and the bowels were freed. After this she was allowed to get up; the urine came naturally by the urethra, and in a few days more she returned home. I have heard lately that this patient has been since safely delivered of a living child, and that she suffered no inconvenience of any kind during or subsequent to her labour.

Case III.—Mary Nolan, aged twenty-three years, after first labour, which was very long and difficult, suffered great soreness of the vagina for some days, and then found a sudden burst of urine through that passage, which has continued to come in that way ever since. Her thighs and buttocks are excoriated. On examination in the position already described, a very frightful destruction of parts was discovered; in fact, the whole front of the vagina was gone, and one looked through the chasm into the cavity of the bladder. The gap extended from the vesical end of the urethra up to the cervix uteri, and to an equal distance transversely. It was nearly square, and measured two inches in every direction. The patient suffered severely from inversion of the bladder, which constantly took place when she stood up or walked, hence she was obliged to preserve the recumbent posture. Notwithstanding the unpromising nature of this case, I was unwilling to allow the sufferer to lose the chance of some relief, and I determined to make an effort to close this formidable breach. It happened that Dr. Battey, of Georgia, U. S., was in Dublin just at this time. He had designed and exhibited to me a modification of the operation of Sims and Bozeman, particularly calculated for this case, and I requested him to visit the patient with me in the hospital.

We agreed to try his operation, which I performed a few days after, in presence of Mr. Pirrie, the eminent Professor of Aberdeen, and author of the great work on surgery, who was on a professional visit to this city at that time, along with my colleagues in the hospital, and some other friends. The earlier steps of the operation were the same as those described; the paring of the edges of the opening, and the passing of the wires were the same. The great extent of the aperture made it necessary to place nine sutures *in situ*. All this was done, and now came the difficulty of closing such an enormous gap; and at this stage Dr. Battey's contrivance came to my aid. A bar of thin lead, two inches long, and one-eighth of an inch wide, perforated with nine holes to correspond with the nine sutures, was prepared; and instead of the second row of holes, as used in the plate in the last operation, nine notches were made in the edge of the plate, corresponding with the nine holes. Through each of the holes was now passed one of the distal ends of the wires, and then perforated shot was run down upon each of the wires, and closed on it by compression with a strong forceps. When all the nine wires were thus secured, the proximal ends which hung out through the anterior edge of the opening were grasped in the left hand and pulled strongly downwards, thus drawing the leaden bar into close contact with the distal side of the opening. The traction was continued and increased, and by degrees the upper edge was made to approximate the lower. This occupied a considerable time, and was much assisted by placing the end of a thin flat piece of wood, like a flat ruler, under the proximal wires, and pressing the end firmly upwards against the part perforated by the wires, while the leaden bar and the parts against which it was lodged were drawn down. By this means the raw edges were finally made to touch. The next point was to secure them in that position. This was done by turning up each wire in succession, and lodging it in the notch in the edge of the bar above described; and when safely lodged there, the edge of the bar was strongly compressed behind it by the point of a strong forceps, thus fixing the wire securely in its place, the distal end already secured by the perforated shot, the proximal end now firmly fastened in the corresponding notch; in this way the nine sutures were fastened. The ends of the wires were now, for further security, twisted by the twister. Some idea of the great difficulty of this operation may be formed from the fact, that three hours and a-half were occupied in its performance. The catheter was introduced and secured as usual, and opium was given as already described. My expectation of success, small

at first, was diminished on the fourth day, when urine was found trickling from the vagina. At the end of eight days the apparatus was removed; the wound was found apparently closed on the right side for a short way; but the strain had been too great on the left, and the wires had cut through the soft parts, leaving a large part ununited. I confess I was not disappointed at the failure in this operation. It was almost impossible to hope for a closure in such an enormous gap at the first trial. The patient remained for some time in hospital, and then went home to the country. She promised to return; and I intend, on some future occasion, to try another operation.

Prolapse of the uterus, when complete, is well known to cause very great inconvenience to the sufferer, and a variety of means have been proposed for its permanent relief; excision of portions of the mucous membrane; destruction of parts of the prolapsed surface by the application of strong nitric acid, &c., have been had recourse to. The latter has been followed by great success in many cases in which I have employed it; but the cure is spread over a very long time, owing to the number of applications that are necessary, and patients get tired of waiting through the long process and the numerous operations. The pessary is only a palliative, and requires looking after and arranging from time to time. The most effectual and speedy remedy for this displacement is that proposed by Mr. Baker Brown, consisting in a permanent closure of the vulva. Two cases were thus treated in the City of Dublin Hospital by me, with complete success.

Case IV.—Mary Kelly, aged sixty-five years, was admitted on account of a very large prolapse of the uterus, with which she had been afflicted for many years. She was the mother of several children, and the prolapse commenced after the birth of the last child, twenty years ago. It had lately increased very much. It was permanently down, not returning when she assumed the horizontal position. The surface was dry, and covered with cuticle, except near the lower part, where some spots of ulceration existed, owing to the trickling of urine over it, and the friction to which it was subjected in walking. As she was a widow, past child-bearing, I considered this a suitable case for the operation of closing the vulva. The woman was kept in bed for some days; the prolapsed parts were returned within the vagina, for the purpose of accustoming them to their natural, but now unusual, position. The operation was performed in the following man-

ner:—The woman was placed on a high table in the lithotomy position, and securely held there close to a window, through which light fell freely on the vulva. I made an incision in the mucous membrane of the labia majora, near the line where it joins the common integument. This incision commenced on the right side, nearly on a line with the orifice of the urethra, was carried all down that side to the fourchette, and up the opposite side to a point corresponding with that from which the incision started. A similar incision, parallel to the last, was made all round at one-fourth of an inch within the vagina. When this was completed, the strip of mucous membrane between the two incisions was carefully dissected off, leaving a raw surface, one-fourth of an inch broad, all round the vulva. Double iron-wires were now passed from side to side by means of needles driven through the common integument, one-fourth of an inch from the cut edge, then through the middle of the raw surface, then across the vulva and through the opposite raw surface, and so out through the integument at a distance from the cut edge similar to that where it had entered. Four of these double wires were thus passed through, and the wound was closed by laying a piece of bougie of proper length along the right side, first between the wires as they emerged from the skin, and making four loops round it by twisting the ends of the four pairs of wires together, and a similar piece of bougie was laid along the left side between the wires; and the edges of the wound being strongly drawn together by pulling the wires, they were twisted together on that side also. A firm quilled suture was thus effected. In order to make the junction of the edges of the integument as complete as possible, three fine iron-wire stitches were made through the skin in the intervals between the deeper sutures. Opium was ordered, as in the former cases; and in order to prevent the contact of urine with the cut edges, the catheter was passed every six hours. On the eighth day I removed the sutures, and was gratified to find the vulva closed by a firm cicatrix, leaving a small aperture at the upper part corresponding to the orifice of the urethra.

Case V.—R. Byrne, aged 60, was admitted in the City of Dublin Hospital, with a very large prolapse of the uterus, which had been down for many years. After preparing her for treatment as had been done in the former case, the operation, as just described, was performed. The same after-treatment was pursued; and, at the end of eight days, a similar happy result was found.

This is an operation that, I think, is only applicable to old

women. I am aware that Mr. Baker Brown proposes to treat younger and married women by an operation similar to that just described, but of less extent,—closing, in fact, only a portion (the posterior) of the vulva. I am afraid such a partial closure would not prevent the escape of enclosed prolapsed viscera; but that bit by bit, and by degrees, the bulk above would insinuate itself into the aperture, and finally escape from the pelvis, as before.

*Case* VI.—In October, 1860, I was requested by Mr. Banon to visit a patient under his care in Jervis-street Hospital. I found her labouring under the distress caused by vesico-vaginal fistula; and thinking it a very favourable case for operation, I placed myself and my instruments at his disposal whenever he thought fit to operate. This he did on the 15th of the month; and he has lately favoured me with the following letter respecting the case:—

"*Mountjoy-square, February* 21, 1861.

"My Dear Doctor,—I send you the particulars of the case of Catherine Ormond, on whom I operated on the 15th October, 1860, for vesico-vaginal fistula, assisted by you; and request you will have the kindness to read them, when bringing the subject forward at the Surgical Society. I take this opportunity of telling you that I had a few days ago a letter from Dr. Riordan, of Bruff, informing me that our former patient, Mrs. Hayes[a], on whom you so successfully operated on the 31st of May, 1859, has recently been safely delivered of a full-grown child, without any injury whatever to the cicatrix of the fistula.

"I am very truly yours,
"A. Banon.

"Catherine Ormond, aged 19, was sent up to me by Dr. Seward, of Caherconlish, and admitted to Jervis-street Hospital, on the 29th of September, 1860. She states, that three months previously she was delivered of her first child, after a very severe and tedious labour, lasting three days; but she made apparently a good recovery, and found nothing wrong in passing water for nearly four weeks subsequently, when she lost all power of retaining it; and it continued to trickle through the vagina ever since, keeping her in a constant state of irritation and misery. On examination, an oval fissure nearly an inch in length, extending obliquely from left to right, was seen occupying the vesico-vaginal septum at its lower part, and having a portion of the mucous membrane of

[a] This was the patient of case No. 2.

the bladder protruding through it. On the 15th of October, the bowels having been previously emptied by a purgative enema, I proceeded to operate, assisted by Dr. Beatty, and in presence of my colleagues in the Hospital. First paring the edges, which I succeeded in doing by removing a complete ring of the circumference of the fistula without a break, five iron-wire sutures were introduced, and secured, in the manner described by Dr. Beatty, on the leaden plate. The patient was kept lying on her face for ten days. A No. 10 male catheter, fixed in the bladder by tapes, and daily changed, conveyed the urine by drops into a vessel suitably placed, so that no accumulation of this fluid could take place in the bladder. Occasional doses of opium were given, and the bowels fortunately did not act during the whole period. The vagina was daily syringed with tepid water. On the eighth day, the sutures and plate were removed, when the fistula presented a smooth appearance, and looked quite healed. On the tenth day, the catheter was removed, and the woman was allowed to get up. From this time she experienced no inconvenience whatever; and left the Hospital shortly afterwards, quite well, the fistula presenting an appearance of firm union. She was recommended separation from her husband for some months. She has recently written to me, expressing her gratitude for her cure."

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Art. X.—*Cases in Surgery.* By Edward Hamilton, M.D., T.C.D., F.R.C.S.I., one of the Surgeons to Steevens' Hospital, and Lecturer on Physiology in its Medical School.

*Double Hare-Lip, which had been submitted to two previous operations without success.*

James Bentley, aged three years, a very fine little fellow, but most wayward and uncontrollable, presented the deformity of double hare-lip in a marked degree; the centre piece extending about half the depth of the fissure. On the left side the cleft was prolonged through the palate; the right alveolar arch was prominent, one of the incisor teeth projecting somewhat into the gap. The segments of the lip were adherent to the gum; the edges did not convey to the touch the usual soft feel, so characteristic of the red border of the lip. The mother states that he had been twice operated on for this deformity: first, at the age of six months, when, according to her account, in two days after the operation, "the needles burst out," leaving

the fissure ununited. He remained in this state until he was eighteen months old, when the operation was again undertaken by another practitioner. She states, that after two days, the needles having been removed, the wound bled freely, and its lips separated,—thus rendering this operation also nugatory. With this state of things, and the history of the case, coupled with the great terror and violent struggles of the little patient, resulting from his painful experience of two previous operations, I felt grave misgivings as to the result, and was kept in continued suspense until the cure was completed.

November 3rd.—The child, having been enveloped in a stout sheet, was entrusted to the care of an intelligent dresser, Mr. Hawkes; the projecting incisor was removed, and the lip freely detached from the gum. The angle of one side was grasped with an artery forceps, which it strikes me is the most convenient instrument for this purpose; the edge was removed with a sharp strong scissors; the central piece was similarly treated. These proceedings were then repeated at the opposite side; the parts were then carefully drawn together, and accurately adjusted with three points of twisted suture. In this operation I was aided by the kind advice and assistance of my esteemed colleague, Mr. Wilmot.

4th.—Every thing progressing favourably. Hainsby's truss was applied, to lessen the strain on the needles.

5th.—The wound presents the appearance of recent hemorrhage. The report states that in the course of the night some bleeding came through the right nostril from the upper part of the wound, occasioned by the child sneezing. This was easily controlled, without resorting to any special mode of treatment. The truss was readjusted.

7th.—This being the fourth day after the operation, the needles were removed, the cheeks supported with broad straps of plaster, and Hainsby's truss readjusted. The wound was well united, except along the right border of the central piece.

9th.—The ununited portion of the fissure granulating. This portion and the needle apertures brushed with a ten-grain solution of nitrate of silver.

In a few days the child left the hospital, with the deformity perfectly rectified.

The clinical teachings of this case present to us, I think, three subjects for reflection. First—The advantage of the early operation, as greatly diminishing the amount of trouble attending its performance. Few, unless actual observers, can understand the trouble of managing this restless little patient. Secondly—The necessity, except in very simple cases, of al-

lowing the needles to remain until the fourth day—a practice so ably enunciated by Mr. Butcher, in the pages of this Journal. I am satisfied, had the needles been withdrawn on the earlier date advocated by some high authorities, nothing could have saved this operation from an unsuccessful issue. Thirdly—How trifling a circumstance may, under Providence, interfere with the success of an operation, however skilfully it may have been planned, or however dexterously it may have been executed.

*Strangulated Femoral Hernia in the male.—Operation, and recovery.*

Thomas Healy, aged 62, admitted November 30, 1860. Complains of obstinate constipation; pain in the abdomen, slightly increased by firm pressure; tympanitis, irritability of the stomach. His pulse is weak and intermittent; in the right groin there is a tumour about the size of a walnut, transversely ovoid, red on the surface, tender to the touch. This tumour is distinctly below Poupart's ligament; not increased by coughing, or diminished by pressure. He states that for twenty years he has had a "small lump" in the groin, for which he could not account, but which caused him no inconvenience. Four days previous to his admission, he was employed shifting some heavy stones; on that evening he passed "some slimy stuff" from his bowels; the next day complained of griping pain in the abdomen. He now observed that the tumour had become enlarged; he had nausea, vomiting, constipation. He continued in this state for two days, when he applied to Mr. Burket, of Tallaght; who, seeing the nature of the case, at once advised his removal to hospital. Being weak after the journey, a little warm wine was administered, and subsequently an enema.

The case was clearly one of strangulated femoral hernia. The tender and inflamed state of the tumour precluded any prolonged or efficient attempts at taxis; accordingly, in consultation with Mr. Colles and Mr. Wilmot, it was determined at once to proceed with the operation. The employment of chloroform was barred by the intermittent character of the pulse. A vertical incision was made by transfixion over the tumour, the layers of fasciæ carefully divided on a director until the sac was reached. This was laid open up to its neck; it contained a little fluid; a portion of omentum much thickened was thus exposed. Careful examination beneath this disclosed a small knuckle of intestine; the point of the finger could distinctly feel

the sharp constriction caused by the crescentic edge of the iliac portion of the fascia lata as it passes to be inserted along with Gimbernat's ligament. Beneath this a fine Cooper's knife was passed; slight *pressure* caused it to yield before the knife. The intestine was now easily reduced, but appeared to be drawn down again by the omentum which was adherent to the outer part of the sac. The adhesion was partly dissected, and a portion of the omentum was cut off; a single vessel required ligature. The intestine, being again reduced, remained so; the wound was dressed in the ordinary way, with a single point of suture, plaster, and a pad. The patient having been removed to bed, a little warm wine and water was given, followed by a full opiate.

December 1st.—Slept during the night. Abdomen soft, free from pain; stomach quiet; pulse weak, but regular; bowels unmoved. To have egg and brandy mixture every second hour. Evening.—Bowels still unmoved; pulse firmer. To have enema of soap and water.

2nd.—Pulse full and strong, tongue dry; some little tenderness about the wound; bowels still unmoved. Omit egg and brandy mixture. Calomel and opium every second hour; half an ounce of castor-oil in peppermint-water as a draught. Evening.—No evacuation from the bowels. The enema to be repeated; warm dressing to the wound. Some scybala passed with the enema, followed in half an hour after by a full motion from the bowels.

3rd.—Bowels moved a second time. Calomel and opium to be repeated twice in the day. To have six ounces of wine.

4th.—Wound still tender; tongue furred, but moist; pulse 90, regular.

5th.—Slept well; no motion from bowels. To have arrow-root.

6th.—No evacuation; tenderness spreading around the wound towards iliac spine. To have three leeches; castor-oil to be repeated. To have stirabout and beef tea.

7th.—Omentum disposed to slough.

17th.—Wound perfectly clean; all tenderness gone; abdomen soft; bowels acting regularly. Omit all medicine; to have chicken.

21st.—Ligature detached; wound healed.

22nd.—Discharged.

Reflecting on the history and progress of this case, we may safely affirm that it was originally a protrusion of the omentum, and that the straining exertion caused a subsequent escape of a knuckle of intestine. I think every candid surgeon will

admit that the operations for hernia which occur in hospital practice are remarkably unsuccessful, even though they enjoy the advantages of the highest operating talent; and this may be explained by the fact, that the majority of the cases which present themselves have been submitted to a long course of treatment in the hope of sparing the patient the pain and risk of the operation. It is only when these means have failed that they are sent to hospital, fairly enough, when we consider the want of proper appliances, and the difficulty of obtaining the requisite amount of care and attention after the operation, which insures its success infinitely more than absurd refinements in the mechanical steps of its performance. If the truth of this statement strikes home to the reason of the surgeon, he may deduce from it the incontestable value of early operation. Far less mischief is likely to accrue from a clean incision, involving even the much-dreaded sac, than the stretching, twisting, and pressure exercised in the process of manipulation, or the lowering of the already too depressed vital powers by the means usually called to the aid of taxis. In this operation the sac was freely divided. This proceeding may draw down the censure of many, the weight of whose authority commands the highest respect; but, reasoning independently for myself from all that I have seen of the operation, and read, as well as from hundreds of dissections of the parts involved, I have found nothing to convince me that a clean division of the peritoneum is as much to be dreaded as all the compression and poking in the dark so often necessary to reduce a hernia through the narrow neck of its sac. A lucid paper has appeared in a late number of this Journal, from the able pen of Mr. Wilmot, whose experience in this operation has been very extensive, in which this matter is forcibly discussed, and placed in a similar light before the profession.

The constriction in this case was caused by the superior external edge of the saphenic opening, sometimes called in this country Hey's ligament, passing to be inserted as described by Mr. Colles, with all the natural truth of that practical writer, along with Gimbernat's ligament, but extending below and external to it. In the division of this structure care was taken to cut by simple pressure, and not by sawing movement. I have frequently demonstrated on the dead subject, in cases of irregular obturator artery, that the fibrous structures may be divided by this method, and the artery borne quite safely on the rounded portion of a Cooper's knife, when, if the sawing movement be employed, it will be divided. The thickened and unhealthy state of the membrane decided us on

cutting it away, in order to diminish the amount of suppuration in the wound.

*Wound by a meat bone, attended with remarkable symptoms.*

James Doyle, aged 29, a butcher, admitted October 25, 1860. Presents all the appearance of extreme prostration. His face is pale and sallow, with an expression of intense anxiety; his pulse is small, and compressible, 110; the surface of the body covered with bullæ, from half an inch to an inch in diameter, some filled with limpid serum, others opaque and puriform, the base surrounded by a red border. His breathing is difficult; the inside of the lips and mouth presents superficial patches, from which the cuticle is detached, but resting loosely on it; his throat is sore, and bloody saliva flows from his mouth. On the middle finger of the right hand is a small suppurating sore, with hard and red margins; the arm and hand are swollen and painful. He states that about a fortnight ago he scratched his finger with a mutton bone; being engaged at the time, he paid no attention to it until an hour after, when, as is usual with the trade, he plunged it into pickle. The day following, he found that the wound was scabbed over; but subsequently some matter formed under the crust, which he punctured with a knife commonly employed for bleeding calves. On the second day after this, he found the wound swollen and red. He applied a poultice, but the swelling extended along the back of the hand. He now felt sick, lost his appetite, but took porter several times in the day. At the solicitation of his friends, he applied to a woman in Church-street, who enjoys considerable reputation for the cure of such sores. She applied some green ointment without any good effect. He was very ill all night, raving and sleepless; and next morning found his hand swollen still more, with great burning pain. He again applied to the herbalist, who advised the ointment to be continued. He now observed red streaks extending up the fore-arm and arm to a hard inflamed spot, above the inner condyle. In this state he continued for two days, gradually becoming worse; the glands in the axilla became inflamed; one or two blisters formed on the painful part of the arm: these he opened, but they soon filled again, and extended over the entire body. He then presented himself for admission. He states that he has been always healthy and sober, never having had venereal; has had two attacks of erysipelas of the head. He was put on tonic treatment; quinia, sulphuric acid, with wine, and sarsaparilla broth.

29th.—The patient presents a truly alarming appearance. The surface of the body has an icteric hue; large bullæ cover

the surface of the body; some have broken, and degenerated into superficial ulcers; the eyelids are excoriated and puffy, with muco-purulent discharge; the nostrils closed with a tough reddish secretion; the breathing difficult. To have sixteen ounces of wine, and half a pint of barm. Oxide of zinc to be sprinkled on the raw surfaces.

30th.—Complains of much soreness in the throat. To have chloride of lime gargle. He continued in this state for some days, when a new train of nervous symptoms set in. He was delirious in the night; he is continually moaning, and rolling his head from side to side on his pillow. If spoken to, he answers in a peculiar loud voice; his respiration is whiffy. Ordered egg and brandy mixture, one grain of calomel on the tongue every second hour.

November 7th.—Distinct evidence of mercurial action on the tongue and gums; delirium less; appears better. Calomel to be administered three times daily.

12th.—Abdomen tympanitic, nausea, urine retained, slight return of the rolling of the head and the delirium. To have turpentine enema; other treatment to be continued.

13th.—Much better; passes water himself. From this report he has continued steadily to improve, although his recovery was retarded by two large bed-sores, which occurred from pressure on the ulcers, notwithstanding every precaution was adopted. His appetite is now amazing. He consumes two chops, half a chicken, two eggs, beef-tea, porter, wine, and a proportionate allowance of bread daily.

If we reflect on the various modes in which this patient was exposed to the contact of animal matter, as well as the symptoms which supervened, we can with difficulty come to any other conclusion than that he received into his blood some poison, which, acting by zymosis on that fluid, produced all this alarming disturbance of the general system. We have, first, the wound caused by bone; secondly, the immersion in brine, in which various kinds of meat not disposed of were, after some time, placed; thirdly, the puncture with the knife, probably stained with animal matter. Making all due allowance for the fallacies of reasoning *post hoc ergo propter hoc,* I think we are justified in assigning blood-poisoning as the cause of the phenomena. The appearances at one time resembled very closely those seen in glanders; but the nasal mucous membrane was engaged only secondarily, and as the result of bullæ forming on its surface, as on the entire tegumentary system. It required a very considerable period to bring the blood to its healthy condition; he was under treatment from October to February following.

Art. XI.—*On the Pathology of Asthma.* By George H. Kidd, M. D., F. R. C. S. I., Assistant Physician to the Coombe Lying-in Hospital; formerly Lecturer on Anatomy and Physiology in the Dublin School of Medicine.

Sir John Forbes, in introducing the section on the Pathology of Asthma, in his article on this disease in the "Cyclopedia of Medicine," remarks, with a truthfulness that is, perhaps, as marked now as when he wrote, that "Systematic and theoretical writers, in attempting to explain the phenomena of asthma, seem to have been misled from the truth by partial views of the subject, and by that aim at extreme simplicity that has so often proved a stumbling-block to medical inquirers. In contemplating the animal system in disease, physicians have been too often disposed to overlook the fact, so obvious in health, that it is by a variety of operations, all conspiring to the same end, that results are produced; and, consequently, that if we attempt to explain the operations of the living body, whether in the healthy or disordered state, on any exclusive or isolated principles, we must inevitably fail."

Since Reisseissen demonstrated the fibres surrounding the bronchial tubes, and Dr. Williams proved them to be actually muscular, and capable of contracting under the stimulus of galvanism, writers—English writers, at least—have been unanimous in attributing the phenomena of asthma to the "exclusive and isolated principle" of the spasmodic contraction of these muscles, and have, consequently, it appears to me, failed to render a true account of the disease. So extremely simple has the explanation founded on this principle seemed, that it has, indeed, proved a stumbling-block to inquirers; and we find that systematic writers have followed one another in describing the phenomena as they should occur in accordance with the theory, rather than in applying themselves to ascertain the facts, and allowing the theory to take care of itself. Thus we find that one author after another describes the thorax as being constricted during the paroxysm of asthma, and sounding badly on percussion, and asserts there is a hollow always to be observed at the epigastrium, produced by the atmospheric pressure forcing the stomach and liver up into the thorax, to fill up the vacuum produced there by the vain efforts of the patient to inspire, while the ingress of the air is impeded by the closed, or at least greatly contracted bronchial tubes; and yet I believe this state of the thorax never exists in spasmodic asthma.

Asthmatics are generally "so well up" on their own cases,

that it is very rarely physicians have opportunities of seeing them during the fit. Indeed, Dr. Watson expressly states, in his Lectures, that since he had become acquainted with the value of the stethoscope, he had never had an opportunity of applying it to the chest of a patient labouring under a paroxysm of spasmodic asthma. So we must not utterly blame writers for copying descriptions of the fit from one another, especially as the symptoms described harmonize so well with the simple theory. Circumstances threw me a good deal into contact, many years since, with one who suffered much from asthma,—one who, from having himself been educated for the medical profession, was well able to analyze his sensations; and by him I had my attention early called to the shortcomings of the generally received description of the symptoms. All my subsequent experience has served to confirm my friend's observations; and I now find that the extended researches of Dr. Hyde Salter prove their correctness. I propose, therefore, to draw attention to the symptoms as they actually exist, and to point out the theoretical views as to the pathology of the disease that they directly teach. This has already been done in some measure, in a review of Dr. Salter's work, which appeared in the number of this Journal for August, 1860; but it seems desirable to do it more definitely in a separate communication than could well be done in a review.

The circumstances to which I would then draw particular attention are, that, instead of the thorax being constricted, and the diaphragm and abdominal viscera being drawn up into the chest, causing a hollow at the epigastrium as generally described as occurring during the fit of spasmodic asthma, it is found, on careful observation, that the thorax is distended to the greatest extent,—that it measures from two to three inches more in circumference than during the interval,—that the intercostal spaces are widened,—that the diaphragm is pulled down to the fullest extent, so as to produce great fulness of the abdomen, and cause the heart's impulse to be felt at the scrobiculus. The sounds, moreover, afforded by percussion over the thorax, instead of being dull are found to be clear, and it is evident that the whole chest is full of air; and on stripping the patient, it is seen, that not only are the muscles of ordinary inspiration in a state of violent action, but that those of forced inspiration are also called into requisition, distending the thorax to the utmost. Dr. Salter's description of this is so graphic that I give it at length:—

"On stripping an asthmatic in the height of a paroxysm, an admirable example is seen of the immense array of muscles

that become on an emergency accessory to respiration; and some idea is formed of the toil of an asthmatic, and the extremity of those sufferings that necessitate for their relief such intense labour. All the muscles passing from the head to the shoulders, clavicles, and ribs, are rigid; and the head is rendered a fixed point, from which they can act on their respiratory attachments. Ordinarily these muscles, such as the splenii and scaleni, have their inferior attachment fixed, and move the head and neck; but now their upper attachment is fixed, and from it they act as mediate or immediate elevators of the ribs, or distenders of the thoracic cavity: and this is how it is that the asthmatic is incapable of moving his head. By the contraction of the trapezius and levator anguli scapulæ, the shoulders are raised to the ears, in order that the muscles that proceed from the shoulders to the ribs may act at an advantage as elevators of these latter. The muscles of the back are so engaged in respiration, that they cease to support the trunk, and so the gait becomes stooping. At every inspiration, the sterno-mastoids start out like cords, and produce by their sudden prominence a deep pit between their sternal attachments. . . . . Meantime all the muscles that increase the capacity of the chest are straining their utmost, and starting into prominence at each inspiration. As each breath is drawn, every muscle is thrown out into bold relief; and since there are hardly any muscles of the trunk that are not mediately or immediately respiratory, the whole muscular system of the trunk may be mapped out in every part of its detail. The straining muscles are rendered all the more conspicuous from asthmatics being generally so thin."

It will be observed from the foregoing—first, that the chest during the fit of asthma is full of air, and distended to the fullest extent; secondly, that all the muscles of inspiration are in a state of excessive and violent action. We have next to remark that there is great difficulty experienced in expiration. Though Dr. Salter adheres to the "exclusive and isolated principle" that asthma depends altogether on the spasmodic action of the bronchial muscles, and does not recognise the teachings of his own clinical observations, these observations are so accurate and consonant with my own, that I shall quote his description in illustration of this point also:—

"In the most intense asthmatic breathing, the difficulty of getting the air out of the chest is so great, the expiratory movement (with all its effort) so slight, and the quantity of air expelled so small, that, as if aware that the chest would never be emptied at that rate in time for the next respiration,

a violent involuntary effort comes to the assistance of the expiration, and pumps out the remainder of the air with a violent jerk. This termination of a prolonged, ineffectual, and almost motionless expiration, by a sudden expiratory jerk, is characteristic of the intensest asthma, and occurs in no other form of dyspnœa whatever. Whenever we see it, we may be sure that the bronchial spasm is extreme. It is at expiration, too, that the asthmatic experiences the greatest distress."

In the appendix to Dr. Salter's book there are several very interesting cases. One of these has been contributed by the gentleman who early drew my attention to this subject. "The balance seemed quite destroyed," he says, describing his own sensations, "between inspiring and expiring. In spite of myself, I was forced to keep tugging *in* the air with all the muscles and joints of my body, while it seemed as if it hardly went *out* again at all. A *cough* as an expiration was a relief,—but a relief that seldom came in those fits till near the end. Besides the peculiar asthmatic sound of respiration, I used to accompany almost every tug for breath with a sort of groan, which (I imagine) helped me, like the *hech* of the axeman, or served as it best might in place of an expiration proper."

In birds, who have no diaphragm, and who have the ribs connected by osseous prolongations with a sternum so large as to cover almost the entire front of the body, the natural state resembles closely that of the human subject when labouring under asthma. "The natural state of this bony frame-work," says Carpenter, "is such, that when no pressure is made upon it, the cavity it encloses is in a state of distention; and the state of emptiness can only be produced by a forcible compression of the frame-work, through an exertion of muscular power. In the state of distention, therefore, which is natural to the cavity of the trunk, the lungs are expanded, and fill themselves with air, which they draw in through the trachea; and this condition they retain till, by the action of the external muscles on the bony frame-work, the cavity of the trunk is diminished, and the air is expelled from the lungs and air-sacs, which are again filled as soon as the pressure is taken off." Now, during the fit of asthma, the state of the thorax and the mode of respiration closely resemble that natural to birds. The chest is distended to the fullest extent, being kept so by the spasmodic action of the muscles of inspiration. When the patient desires to breathe, he must first bring a strong voluntary effort to the aid of the muscles of expiration, so as to compress the thorax, and as soon as he relaxes this, the muscles of

inspiration, being in a state of tonic or persistent spasm, expand the cavity again, and cause fresh air to rush in through the trachea. In the healthy state of the human subject, there is always a rest after each expiration. We have first an inspiration, then an expiration following immediately, and then a post-expiratory rest occupying a time considerably longer than that of the inspiratory and expiratory movements combined. In asthma, this post-expiratory rest is lost; the moment the expiration is completed, the inspiration begins—there is no pause. In fact, as has been already remarked in the review of Dr. Salter's work, it seems plain that the muscles of inspiration are, during the fit of asthma, in a constant state of tonic contraction; and that the great difficulty is to empty the chest, so as to allow of its being refilled with fresh air. For this purpose, the muscles of expiration are constantly contending with those of inspiration, and require the aid of a strong voluntary effort to overcome them. It appears, moreover, that as soon as the volition is suspended, the inspiratory muscles immediately regain the victory,—the chest is again distended; the moment the expiration is completed, the inspiration begins,—there is no pause,—the normal post-expiratory rest is lost. In consequence of the state of distention in which the chest is during the fit, and of the necessity of overcoming the tonic spasm of the inspiratory muscles, it occurs that the relative length of inspiration and expiration is reversed; instead of the inspiration being longer than the expiration, as is normally the case, the expiration is longer than the inspiration,—four or five times as long; and at the same time the respiratory movements are unfrequent, not more than nine or ten in the minute; and from the state of distention in which the lungs are kept, it also arises that the vesicular murmur can only be heard on desiring the patient to make a prolonged and forced expiration, and then inspire quietly, as has been remarked by Lænnec and Williams.

The desire manifested by asthmatics for an abundance of fresh air during their fits, and their violent inspiratory efforts, seem at first sight to be opposed to this theory, but in reality are best explained by it. Owing to the persistent distention of the thorax, the lungs are filled with air, which, from being stagnant, soon ceases to be of avail for purifying the blood, and hence the "*besoin de respirer*" is felt in an aggravated degree.

While an examination of the phenomena of the asthmatic fit proves that the entire muscles of inspiration are in a state of spasmodic action, many circumstances show that the bronchial muscles are also concerned, and are, too, in a state of

spasmodic action. This is most easily demonstrated in the slighter attacks, where, on applying the ear to the chest, it is found that the respiratory sounds are heard in an undulatory sort of way, marking the dilatation and contraction of the bronchial tubes, and that the points of constriction are constantly changing their site.

There are few problems in physiology more difficult of solution than that of the action of the bronchial muscles. When their existence and muscularity were first demonstrated, it seemed natural to regard them as muscles of expiration, serving by their contraction to expel the air from the lungs; but a little consideration shows they cannot take part in this process. Their distribution alone will prove this—they are found in the trachea, and larger bronchial tubes, occupying the spaces where the cartilaginous rings are deficient, in the posterior fourth of their circumference, passing transversely between the extremities of each ring; in the smaller tubes, where there are no cartilaginous rings, they surround the tubes, and may be demonstrated on the finest tubes, but they do not surround the air-cells or terminal portions of the tubes. Now, these air-cells are the essential part of the lungs—they are expansions at the extremities of the bronchial tubes; it is in them that the blood is aërated; they are much larger in capacity than the bronchial tubes, and these tubes are but ducts for conveying the air to and from the air-cells. The air-cells are emptied by the collapse of the walls of the thorax; and it is evident that if the ducts were narrowed or closed by the bronchial muscles while the air-cells were being compressed, the effect would only be to impede or prevent the egress of the air, and perhaps rupture the cells. We must, then, give up the theory that they are muscles of expiration. It has been suggested that they are used merely to carry forward the mucus secreted in the tubes by a sort of peristaltic movement, and moreover to regulate the quantity of air to be admitted into each lobe of the lungs. The necessity for this latter is not very obvious; and it is plain that where there are cartilages in the tubes, the muscles cannot narrow their caliber sufficiently to aid very much in the expulsion of mucus—and any explanation, to be good, must apply to their entire distribution.

The theory as to their use that seems to me to accord with the greatest number of facts is, that they are muscles of inspiration, and, if this can be proved, it will afford a satisfactory explanation of their being associated with the other muscles of inspiration in the production of a fit of asthma. The principal fact on which this theory is founded is the difference between

the duration of inspiration and expiration. On analysing our own sensations, or applying the hand to the chest of a healthy person, it is evident that the movement of expiration is much shorter than that of inspiration; and on applying the ear to the chest, the inspiratory murmur is found to be much longer than that of expiration. Fournet, who attached much importance to the relative length of these two sounds as a means of diagnosis, says that in health the inspiratory sound is five times longer than the expiratory, and for the purposes of clinical observations proposed to name them as 10 and 2. Now, if a volume of air occupies a certain length of time, which we may call ten, in passing into the lungs, and passes out again in the one-fifth of that time, or in a period that we may call two, it is evident it must either be pressed out with greater force than it passes in, or the tubes through which it passes out must be wider than those through which it passes in. One or other of these propositions must be true—

1st—Either the force of expiration must be many times greater than that of inspiration; or,

2nd—The bronchial tubes must be narrower during inspiration than they are during expiration—*i. e.*, the bronchial muscles contracted during inspiration and the tubes narrowed; the muscles relaxed during expiration, and the tubes dilated.

It is well known that the resistance offered to a current of air passing through a tube increases rapidly, in proportion to the velocity of the current. The correct estimation of this resistance is one of the most difficult problems in dynamics. Newton considered that the resistance varied generally as the squares of the velocities of the currents; but it is well known that in small tubes, and with great velocities, the resistance is much greater than this. By a simple experiment, I have attempted to form some estimate of the amount of force that would be required to cause a current of air to pass out of the bronchial tubes five times faster than it passes into them. Having accurately balanced the receiver of Hutchinson's spirometer, I suspended it by a single cord, passing over a pulley; to one end of this cord the receiver was attached, to the other the balancing weights; I then lowered the receiver into the water, and having slightly opened the air-tap I attached weights to the cord till I caused the receiver to rise, drawing air in through the air-tap. I found that a weight of 1lb. caused the receiver to rise so as to draw in 400 cubic inches of air in 60 seconds; and I formed the following table:—A weight of 1lb. caused 400 cubic inches of air to pass through the narrow opening of the air-tap in 60 seconds, and

a weight of 2lb. caused it to pass through in 40 seconds, and so on.

| | |
|---|---|
| 1 lb . . . | 60 seconds. |
| 2 „ . . . | 40 „ |
| 4 „ . . . | 25 „ |
| 6 „ . . . | 19 „ |
| 8 „ . . . | 16 „ |
| 9 „ . . . | 15 „ |

Showing that it would require the force moving a current of air through such an aperture to be multiplied by 9 to increase its velocity fourfold.

This experiment may be varied so as to cause the velocity of the current to approximate to that of the current passing into the lungs in ordinary breathing. The quantity of air passing into the lungs in ordinary breathing has never yet been determined; but it is estimated by Coathupe, whose experiments are considered most worthy of confidence, at about 20 cubic inches: now, say that this occupies a second, or a second and a half, in passing into the lungs, it is obvious that if we arrange the spirometer so as to draw in 333 cubic inches in 25 seconds, the air will have about the same velocity as that passing into the lungs. I opened the air-tap of the instrument so as to allow the receiver to rise to 333 cubic inches, with a weight of 1lb., in 25 seconds, and then added weights till it rose to the same height in five seconds, when I found I had been obliged to put 10lbs. in the scale.

I infer from these experiments that, to cause the air to pass out of the lungs in the one-fifth of the time it occupies in passing into them, the diameter of the bronchial tubes remaining the same, it would be necessary that the force of expiration should be at least ten times greater than that of inspiration. But it has been proved by the experiments of Mendelssohn and Hutchinson (these last extending over 1500 individuals), that the expiratory force is only one-third greater than the inspiratory force. We thus eliminate the first proposition, and are reduced to the second—that during inspiration the bronchial muscles are contracted, and the tubes narrowed; and during expiration the muscles are relaxed, and the tubes dilated.

The mode in which the contraction of the bronchial muscles assists in inspiration is easily explained. The object of inspiration is to carry a current of air into the air-cells. This is effected by enlarging the cavity of the thorax relatively to its contents—1st, by dilating the cavity; 2nd, by lessening the bulk

of its contents. This latter is effected by the bronchial muscles, which narrow and shorten the bronchial tubes, leaving extra space for the enlargement of the air-cells. The narrowing of the tubes assists inspiration materially in another way. Though in ordinary respiration there be but twenty cubic inches of air introduced into the lungs and expelled again, we know that in health a quantity of air remains in the lungs after ordinary expiration, varying in different subjects between 117 and 430 cubic inches. The greater part of this is lodged in the air-cells, under circumstances most unfavourable for its displacement. By the narrowing of the tubes, the displacing force of the current will be greatly increased, and thus important aid given to the essential part of respiration. So difficult does it appear to effect the displacement of the contents of the air-cells, that it is stated by Dr Walshe, in his treatise on diseases of the lungs, that in calm ordinary breathing the air in the air-cells is stagnant and unchanged, the tidal current never reaching it. On such a supposition, the greater part of the lungs would be useless, or worse, for it would contain impure air—air unchanged, except so far as could be done by the tendency of the tidal air and it to become mutually diffused. The action of the bronchial muscles now described, however, obviates the difficulty; for, by narrowing the tubes, they cause the current to acquire a great increase of displacing power, and this is probably the most important use of these muscles.

Emphysema of the lungs is one of the most constant results of asthma, and can be best accounted for on the supposition that the chest is in a state of distention during the fit. Dr. Gairdner has shown that emphysema is produced by the over-distention of some of the lobules of the lung during inspiration, caused by their swelling out, to occupy the space of other lobules to which the access of air is prevented by a plug of mucus, or other closure of a bronchial tube. We have but to suppose the spasm of the bronchial muscles to be so complete as to close some of the tubes, when the greatly-distended thorax supplies the condition essential for the production of the emphysema.

I have now shown,—

1st,—That during the paroxysm of asthma, the chest is distended to the greatest possible extent.

2nd,—That all the muscles of inspiration are in spasmodic action (tonic spasm).

3rd,—That the bronchial muscles are muscles of inspiration, and associated in the spasmodic action with the other muscles of inspiration.

4th,—That breathing is carried on by bringing a voluntary effort to aid the muscles of expiration; and that as soon as this is relaxed, the muscles of inspiration, like so many stretched bands of Indiarubber, distend the chest again.

I proceed now to trace this spasmodic action to its cause. The "simplicity" of the theory, that the paroxysm depended solely on the spasm of the bronchial muscles, has too often prevented the true cause of the disease being recognised. The following facts may be referred to, as showing that the spasm arises from some morbid action in the medulla oblongata:—

1st,—The fact that the spasm affects an entire group of muscles. Now, Schrœder van der Kolk has shown that muscles which are associated in action are supplied by nerves arising from special groups of mutually associated and connected ganglion corpuscles. Disorder of this group would then manifest itself in the entire class of muscles.

2nd,—Van der Kolk has also shown that the skin covering parts moved by muscles, is supplied with sensitive nerves arising from the same segments of the spinal centre, as the motor nerves of those muscles arise from[a]. Now, Dr. Salter has remarked, as an almost universal premonitory symptom of asthma, that there is *itching* of the skin under the chin, over the sternum, and between the scapulæ. This, it is evident, is a subjective sensation, and indicates an irritation existing at the roots of these nerves.

3rd,—Paroxysms of asthma are observed to occur in cases of acute hydrocephalus, as in a case mentioned by Dr. Salter, and in one mentioned by Dr. Graves, where there were also general convulsions. In persons liable to epilepsy, recurring at regular intervals, fits of asthma occasionally take the place of, and serve as substitutes for, the epileptic fit.

4th,—The state of the patient preluding the fit of asthma indicates an affection of the nervous centres. In one there is mental exhilaration, in another mental depression. A patient of Sir J. Forbes is awakened from sleep by convulsions in one foot and leg; and as soon as the asthmatic fit is developed, the convulsions of the extremity cease.

5th,—The exciting causes indicate the same. In one, cold water applied to the instep will cause an attack; in another, going to bed with a loaded rectum; in a third, undigested food in the stomach; and in another, sudden emotion; which latter

[a] There are many interesting illustrations of this law in a note, at page 7, of the edition of van der Kolk's works published by the Sydenham Society.

will also sometimes check the paroxysm, even when fully developed.

From all these circumstances, I infer that asthma depends on a morbid state of the medulla oblongata and spinal centres, which manifests itself by throwing the entire group of inspiratory muscles into spasmodic action.

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Art. XII.—*Further Remarks upon a new and successful mode of treatment for Vesico-Vaginal Fistula.* By Maurice H. Collis, F. R. C. S., Surgeon to the Meath Hospital.

In a former number of the "Dublin Quarterly Journal," I drew the attention of hospital surgeons to a new method of operation in cases of vesico-vaginal fistula. I claimed for it some advantages over older methods, which had proved unsatisfactory in their results—advantages which a more extended experience has shown to be its just due, and which the cases I am now about to detail will be found to illustrate. The operation consists, first, in splitting the margin of the fistula all round, so as to separate the vesico-vaginal septum into two equal portions, one-half consisting of the vaginal mucous membrane and submucous tissue, and the other of the vesical mucous membrane and submucous tissue, the muscular portion of the septum being equally divided between the two. The extent of this artificial separation is to be regulated by the extent of the fissure, by the condition of the margins, and to a certain extent by the position of the fissure Where the fistula is near the vesical end of the urethra, or near the cervix uteri, the dissection need not be carried to any great depth—in the former case from the substantial and highly vascular condition of the parts which insures ready union, and in the latter from the toughness of the structure, and also because there would be some risk of opening into the canal of the cervix, an accident which would give rise to a troublesome complication, in the form of a fistula, most difficult to close. On the other hand, when a fistula is situated in the intervening space or floor of the bladder, the dissection requires to be somewhat more extensive, owing to the thinness of the septum, which would otherwise afford too small an extent of raw surface to secure union.

The condition of the margins regulates the amount of splitting in this way—if the margins are unhealthy, or infiltrated, or unnaturally thinned away, the separation of the

flaps must be proportionately extensive; if they are healthy, a less amount of dissection suffices. The measurement of the fissure has also a bearing on this point, as stated above; if it be extensive, the flaps must be ample; if small, they need not be large.

The second step of the operation consists in the insertion of the sutures; for my operation either wire or thread will answer; silk is now justly condemned by most surgeons as irritating, and productive of ulceration, as a suture. Good thread has always seemed to me preferable to iron-wire. I have not an equal experience of silver-wire, and cannot speak to its merits; but I have frequently found iron-wire as irritating in the vagina as silk, although in cases of cleft palate it will remain for weeks unaltered and harmless. I have even gone to the trouble of securing the same kind of wire as Professer Simpson uses, but without having the satisfaction of obtaining the same immunity from sloughing which it seems to possess in his hands. I cannot account for this fact, and only state it that others may not be disappointed Thread, on the other hand, seems not to produce much local irritation; and I have had thread-sutures lying in the vagina for ten days without producing injurious suppuration. The needles which I use are known as Liston's needles; they are fixed in long handles, with the eye near the point; with these it is easy to pass the thread; and if wire be used, it can be drawn through the flaps by attaching it to the thread. I have my ligatures about a quarter of an inch from each other, and at a considerable distance from the margin of the fistula; this is of great importance. Each suture consists of a doubled thread; and when all are passed through the flaps, a piece of vulcanized India rubber cord is run through the looped extremities, which are successively pulled tight upon it, and the free ends are then tied over a similar piece of cord at the opposite side of the fissure; the sutures are not tightly drawn. The degree of tightness with which they are to be tied, and the selection of the distance from the margin of the fistula at which to enter them, will test the judgment of the operator, and will most probably insure success or failure as far as he is concerned. If the raw surfaces be tightly drawn together, so that their edges puff up between the India rubber quills, the flaps will slough. The operator must remember that much swelling will inevitably arise in the course of twenty-four hours, and that the effect of tightly confining the flaps between two bars will be to interfere with their nutrition, and bring about their death. In like manner, if the quills are too close to the margins, their

pressure will produce a similar destructive effect. If there be any strain upon the quills, they must produce some slight ulceration where they lie in contact with the vaginal surface; and if the strain be too great, or too close to the margin of the fistula, they will cut through, and cut off the flap. I have seen this happen in other hands, in spite of warning; and it has happened in my own very lately, owing to my using the hemp ligature, which, when wet, contracts most powerfully. I was not aware of this, and was seduced into its use by its beautiful roundness and smoothness, to meet with a result which was by no means agreeable. In my first case I used for quill a piece of soft black bougie, for which, in subsequent instances, at the suggestion of Dr. Thorpe, of Letterkenny, I substituted the India rubber cord. Its advantages are its elasticity, which enables it to give a little in case of unexpected swelling, and its pliability, which enables us to adapt it to any inequalities of the margins of the fistula. This was strikingly illustrated in Madden's case (No. 3), where the rent was irregularly crucial, and where no unyielding bar would have suited.

Such are the steps of my operation; and I shall now briefly explain in what particulars it seems to be an improvement on the ordinary and older method.

*First*—It affords an extent of raw surface far and away greater than where the margin is simply pared. It is manifest that any reasonable extent of surface can be obtained in this way; it is only necessary to extend the ring of dissection, to obtain this end; within reasonable limits there is no objection to this procedure. The ridges which might be supposed to result, and which do in fact exist when the cure is complete, will in a few days be smoothed away, and be productive of no permanent inconvenience.

*Secondly*—This extensive surface is obtained without the loss of a particle of structure, whether in small or large gaps; this is of inestimable advantage. Any one who has removed a ring of mucous membrane from ever so small a fistula, will have observed how large it becomes under the process, and sooner or later will have probable cause to regret the loss of substance thus entailed; whereas, in my operation, if carefully done, no loss of material occurs; and even if it fail, it leaves the patient in no worse condition for subsequent treatment than before. There are, as I shall presently show, cases in which my operation is not expedient, and in these it may be necessary to sacrifice material; but, with these exceptions, I do not think a surgeon is justified in selecting any mode of

operating which necessitates the removal of material where there is already a deficiency, and when other methods, which give at least as good results, are offered for his use.

*Thirdly*—In this operation, we have a double prospect of success. It will be seen, when the raw surfaces are drawn together by quilled suture, that a ridge rises up on the vaginal surface between the quills. A similar and larger ridge is thrown up towards the bladder; this acts as a valve to prevent the water escaping, or even coming in contact with the wound. And even if the parts included between the quills should slough, the flaps which point towards the bladder are exempt from the pressure of the quills and the strain of the sutures, so that they escape from sloughing, and actually unite before the other flaps, and remain united, even if the latter give way.

*Fourthly*—The operation is simple, and requires no very complex armamentarium, and only that amount of dexterity which should be possessed by every surgeon deserving of the name I generally prefer the lithotomy posture. It admits of the use of chloroform, and in most instances the fistula can be brought into sufficient command. Sometimes, however, I have found it expedient to adopt another position, viz. placing the patient standing with her back to me, and making her lean across a table, as in the position for examination of the rectum. I use Dr. Sawyer's divaricators, which are of strong brass, bent at about four inches from the end, at right angles; two of half inch width for the sides, and one of inch width for depressing the rectum. My knives are slightly bent on the flat, an inch from the point, and only differ from those figured by Simpson in being double-edged. One has a lancet point, the other has the angle rounded off; the former is useful in commencing the incisions, the latter in continuing them. However, if such knives are not at hand, any pocket-case will supply the want; for it is not so much the implement as the hand that guides it that secures success; and all surgeons ought to be able to do their work with as few implements and as simple as possible. A good sharp scalpel will do the work as well, if no more convenient knife should be at hand; and a few common iron spoons, properly bent, will make divaricators at a pinch. I have even known an operator pass sutures with common curved needles held in a dressing forceps, when those with which he had supplied himself became bent or broken in the progress of the operation.

*Lastly*—My operation is suitable to almost every case, and to many cases which could not be subjected to the older methods with the smallest chance of success.

In a large gap, where the loss of substance is to be measured by square inches, no person could expect union by simply paring the edges and drawing them together by the interrupted suture. The strain on the threads would be too great, and they would inevitably cut out. Nor is the success of autoplastic operations, by which flaps are transplanted from neighbouring parts, such as to lead us to expect much from them. In these cases it is of great importance to have a mode of operating which can be frequently repeated without repeated diminution of the already scanty material. In more than one instance I have succeeded in gradually reducing the aperture to moderate dimensions by a succession of operations, and finally have closed it completely. I have invariably included the entire margin in the first operation, as it is impossible to say what part or how much may unite at once. The advantage of this appeared in one case, by the almost complete closure, at one operation, of a fissure which measured three inches in length; a single point only remained open, and this subsequently contracted so as to cause no further inconvenience. In another case, one-third of a large gap closed at the angles; and a subsequent case of almost equal dimensions was rendered manageable by the centre portion uniting firmly, and so subdividing the fissure into two portions, at which I was enabled to work alternately without loss of time as well as without loss of substance.

In small gaps, on the other hand, it will not redound to the credit of the operator, if the rent is made worse each time that he interferes. Such a misfortune cannot happen by my operation in any case to which it is suited. There is no loss of substance; and the surgeon can begin again *de novo* in a few weeks, with the parts in at least as favourable condition as before.

In comparing it with Bozeman's method of operating, it will be found to possess both advantages and the opposite; but these will be more intelligible when I have explained, as I shall now do, the treatment of a case subsequent to operation. The vagina is washed out with cold water; a catheter is passed for a moment to empty the bladder of any few drops of urine that may have collected, and it is then withdrawn until the patient has been placed in bed. If the fistula inclines more to one side than the other, she is placed on the side that is farthest removed from the fistula, with instructions to the nurse to keep her from turning over on her back, as patients are apt to do while recovering from chloroform. As soon as she is conscious, she is similarly cautioned; and is further advised to lean over as much on her face as she can. In this position she is kept for

at least two days. The catheter is again inserted immediately on her being placed in bed, and a small vessel laid beside her to catch the urine as it drops from her. I have used various catheters, and find the ordinary male gum-elastic the best, for more than one reason—it is long, soft, pliable, and you can insure its cleanliness by having a new one for each case. Further, I have found the urethra less impatient of it than of metal instruments. In any case, it should be carefully watched for the first day, lest a clot of blood should get into the eye and check the flow of water. In case the urine is not seen to drop from it, it should be gently drawn back and forward once; if the urine does not then flow, it should be withdrawn entirely, cleared out, and re-introduced[a]. At the end of two full days (forty-eight hours), the patient may be turned carefully on her back, the bed having been wheeled opposite a good light, and with the fingers, or with the gentlest and most cautious use of one or two divaricators, the quills and wound may be brought into view with the smallest amount of disturbance possible. If the wound looks healthy; if the flaps are not blue from strangulation (over-tight sutures); if the quills appear not to have corroded the mucous membrane on which they lie, the fingers and divaricators may be carefully withdrawn, the patient turned on the opposite side, a fresh catheter introduced, and all things left as before.

The next day it will be well to repeat the examination, and, if necessary, to cut the loops of the sutures, or at least of the alternate ones. This is best done by touching them with a sharp knife as they cross the quill, cutting them on the quill in fact, and using no force, no pulling or dragging of the quills, and no too curious inspection to see if union is perfect. On the next day the remaining sutures (if any) are to be divided, and one quill removed if loose; and on the succeeding day the remaining quill and sutures will probably be found loose in the vagina, and may be removed. At each inspection it may be well to syringe out the vagina with tepid or cold water. After the third day the catheter had better be withdrawn, when the urethra will be found patulous. The catheter may be introduced morning and evening, to insure the complete emptying of the bladder; or at any time, if the urine should

[a] A little wine should be given as soon as the sickness from the chloroform is passing away; and when the stomach is settled, a grain of opium, in pill, should be administered, and be repeated night and morning; arrow-root or rice should be given as food, and the patient may be allowed to drink as freely as she wishes, the dilution in the urine counterbalancing any increase in its amount. I have found by experience that heroic doses of opium are unnecessary to insure quiet in the bowels, and on one occasion they produced positive mischief by inducing vomiting and diarrhœa.

be felt or seen to cease from flowing. I have found, as a general rule, that it irritates the urethra so much as to induce suppuration, if the instrument be kept in longer than three days; and the suppuration is apt to extend toward the wound, and may even give rise to subacute cystitis. Hence I recommend and practise its early removal. After the quills have been removed, great care is required for two or three days, lest any accident should rupture the soft union. Vomiting, diarrhœa, incautious movements, and accidental injuries by the catheter or syringe, might do irremediable mischief; but, without some such cause, there is no likelihood of the union giving way. At first, a thickish ridge will mark the site of the line of union, and two lines of raw surface will show where the quills had lain; but in a very few days these will have healed up, and the former will have disappeared. And I think that we may fairly conclude that the ridges on the vesical surface follow the same course. For a variable period incontinence of urine will probably exist. Of this there are two causes—the patulous condition of the urethra, and the loss of dilating power in the bladder. Both are remediable by time and patience; a few words of advice to the patient to make water often at first, and by degrees to try and retain a little more in the bladder, will counteract both causes of incontinence. For obvious reasons, the patient should be kept in hospital, or at least should not be allowed to return to her household duties, until the cicatrix has acquired complete solidity; or if such arrangements be not practicable, strong cautions should be given to all parties concerned to avoid for a time everything which could injure it. An example of the mischief of premature coitus will be found in one of the following cases, where, from the circumstances of the case, the usual prohibition had not been given.

As stated above, there are points in which my operation and Bozeman's may be advantageously compared with one another. Where a surgeon possesses judgment, and nicety of touch, he will find my operation afford him excellent results in most cases. He requires both qualities for deciding on the depth to which the splitting process should be carried, the degree of tightness with which the ligatures should be tied, and the moment when to relax them. Bozeman's operation is more mechanical; and, though complicated and troublesome, when once the details are mastered, and the apparatus is bought, it needs dexterity of hand alone to insure reasonable success. For reasons I have given a few pages back, it is as little applicable to large rents as the older method, on which it is an in-

genious improvement. Its great advantage—and to my mind a very moderate one—is the comparative safety with which the sutures can be removed. The shield protects the cicatrix from injury at the moment of their division; and, as they cause no irritation, they need not be removed for several days, until all is perfectly firm. My operation is done much more quickly, and has been completed in periods varying from twenty minutes to an hour and a quarter. I have heard of Bozeman's occupying three hours and a half. There is, however, a form of fistula in which I prefer, and always perform the latter—where the edge of the fistula is funnel-shaped and puckered. Such fistulæ are small, and generally high up. The condition of the margin makes it almost impossible to apply my plan of splitting; and I find it more expeditious and safer to pare away the mucous lining of the fistula and its funnel-shaped mouth, and to insert wire sutures, and apply Hardy's modification of Bozeman's button. In this we have two parallel rows of holes close together; and the ends of each wire, being passed through two neighbouring holes, are simply twisted together, instead of being clamped with shot. I have even applied this plate to the palate in a case of syphilitic loss of substance, in the hope that it would effect a closure. The case was not favourable for success, and only partial improvement followed; but sufficient to show me that most important assistance could be had from a well-adapted button, in suitable cases of fissured palate.

I shall now proceed to detail the cases in which I have operated, chiefly by my own method, and sometimes with Bozeman's. A tabulated arrangement will give the results in a form available at a glance to those who may not care to wade through the history of each case from day to day. It will be seen that some of the patients are still under treatment; but as I wish to give every information, and to leave no loophole for cavilling at the results obtained, I have been perfectly explicit. I have only to add that I have met with but one case in which, though I did not refuse to operate, I thought the chances so small, that I did not press the operation. This was a case of sloughing, which had destroyed every vestige of the floor of the bladder, and left nothing but the sides of the vagina from which any flap could be obtained. The chances of success were next to none, and did not justify me in pressing the operation on an unwilling patient. But, short of this, I believe there is no gap which is so bad that an ultimate cure may not be obtained by patience. Nothing could be more unpromising than some of my cases, and nothing more satisfactory than some of these turned out. One case at present under treatment was apparently quite hopeless when we commenced

operations. The gap was large enough to admit four fingers; the patient a small woman, with small vagina: and yet, after three operations, this huge gap is reduced to a mere slit, of a few lines in length, which I have reasonable hope of closing in another operation. In these large cases, one must be content to cure bit by bit; although, as I said before, it is well to give the entire margin the chance of uniting each time.

Case I. (Mrs. Giblin, aged 25) is published in the "Dublin Quarterly Journal" for February, 1857. I have to add to the published record that the woman has since been delivered, and that the cicatrix (a transverse one, situated at the junction of the urethra with the bladder) has stood the test of the subsequent parturition uninjured. The cause of the injury was protracted labour in a primipara. The fissure was one inch and a quarter long. The operation was done five or six weeks after delivery, and consisted in splitting the edges of the fissure all round, and uniting the raw surfaces by quilled sutures of common housewife-thread, the quills being pieces of French No. 6 bougie. Ligatures were removed on the fifth day, when union was complete. She speedily gained perfect control over the bladder.

Case II.—Mrs. Balfe, aged 40, also published in the same number of the "Quarterly Journal," was one of the most satisfactory cases I have had, so far as the amount of relief obtained from the utmost misery. This woman had a rent three inches long, extending obliquely from a point close to the meatus, back to the immediate neighbourhood of the os uteri. Through this chasm the upper wall of the bladder prolapsed to such a degree as to appear externally. She could neither sit, stand, nor walk without pain: as her husband expressed himself to me, she lay for two years on a sack at the fire, not able to do a hand's turn for herself or any one else. In one operation this large rent was closed, with the sole exception of a small hole at its posterior angle. She experienced such instant relief from her acute sufferings, that she refused to undergo a second operation for the closure of this small pinhole, and her subsequent history has proved that she was not far wrong. I have heard from her within the last month, and she states that she suffers no inconvenience, and that, unless after a long journey, she has perfect control over her bladder. The operation in this case was the same as in No. 1. The causes were prolonged labour and delivery with the forceps. It was her sixth child.

Case III.—Bridget Madden, aged 30, a soldier's wife. She stated that a surgeon (Scotch) made an incision with a razor

into what she thought were the membranes, but in reality into the vesico-vaginal wall. Whether this be true, or one of many inventions of a not very truthful female, I cannot say. Certainly there was a rent of irregular outline, and, taken with cicatrices which existed in its neighbourhood, it bore every appearance of a crucial incision having been deliberately made in this situation. I operated on her four times in February, March, May, and June, 1857. The first, second, and fourth by my own method, and the third by Bozeman's. Finally, she was dismissed perfectly cured, and able to retain her water for three hours at a stretch. Sir P. Crampton and Dr. Beatty, among others, saw this woman, and were much pleased with the result. However, in a few weeks, she presented herself to Dr. Johns during my absence from town, and complained that she was as bad as ever. Upon examination, this proved to be true, in so far that there was an opening in the centre of the cicatrix, of an oval form, about as large as a No. 10 catheter, and with edges no thicker than paper. Careful inquiry proved that she had taken up with another soldier, her proper husband being in India, and in this way the rent had occurred. On my return she applied to me, but under the circumstances a fresh operation appeared not likely to have better success, and I declined any further interference at that time. I have heard nothing of her since.

Case IV.—Mary Levey, from Portarlington, sent up to me by my friend Dr. Tabuteau. This woman had a rent made by the violence of a midwife in her first confinement. She had six children before I saw her. It was a small transverse rent in the floor of the bladder; the walls were thin. I reduced it by my first operation to the size of a crow-quill; by a second operation it was further diminished, and became so small that I thought to close it by a simple wire suture. This failed, as did also a hare-lip suture over a short small needle; and finally, I tried Bozeman's button with equal want of success. I attribute these failures to overmuch haste in repeating the operations before the edges of the fistula were restored to a healthy state. At last our patience became mutually exhausted, and she left me with a pinhole so small as only to admit of one of Anel's probes. I had tried cautery and caustics to close this, but without avail. Yet this minute fistula was sufficient, from its situation in the centre of the floor of the bladder, to render her as unable to retain her water as the larger aperture with which she first applied to me. She has since had a miscarriage and a labour at term, and expresses to Dr. Tabuteau her determination to come up again when the

child is weaned, so that I may have one chance more of rescuing this from the list of failures.

Case V.—Catherine Murphy, from Dingle, sent up to me in 1859, by Dr. Alton, of Tralee, to whose kindness I am indebted for the previous details and subsequent history of the case. This poor woman was delivered in a remote part of the country, unaided, and suffering from puerperal convulsions. She recovered with sloughing of the vagina, and occlusion of the passage high up, together with a very considerable vesico-vaginal fistula. I operated on her after my own method on the 11th May, 1859, and as usual removed the sutures on the fourth and fifth days. Union was then perfect; but by an error of the nurse she got an over-dose of opium which brought on violent vomiting. The bowels also gave way at the same time, and severe diarrhœa occurred on the evening of the fifth day; hence a great portion of the soft cicatrix separated, and we had to begin afresh. Early in July I again operated, and this time by iron-wire sutures and Hardy's modification of Bozeman's button. The result of this second operation was so far favourable, that Mrs. Murphy could retain her water for hours while quiet, or for some minutes while moving about. There was still, however, an opening the size of a grain of duck-shot. She left for the country; and circumstances brought me down to Tralee in the ensuing September, when I saw her at Dr. Alton's house. We then cauterized the fistula, which was small and puckered, and hard to discover among the folds of the vagina. I did not see her again, but Dr. Alton writes me that she called on him some time afterwards, and the orifice appeared to be completely closed and a cure effected, as she told him she could retain her water very well, and there was no stillicidium. Her clothes and person had no urinous odour. Now for her subsequent history (I quote from his letter):—"Some months after she called upon me again, and at this visit only to determine the fact of pregnancy, or not. You may recollect at the period of our seeing her here that the vagina was completely closed, with the exception of a very minute orifice, sufficient barely to allow of the introduction of a bougie, and there was no possibility of introducing the finger into the upper portion of the vagina, so as to reach the os uteri. However, upon the occasion of this visit, I found her three months pregnant, the vesico-vaginal fistula seemed completely closed, the site was marked by a very small pucker, as if all the coats were not quite united. She stated she was never wet, but passed the urine a little oftener than she used before her confinement, or when she was quite well. On passing the

finger I could feel that the vaginal surfaces were more apart and bands becoming elongated, but still could not reach the os uteri. I directed her to call again, which she did in November last. She seemed to be cured of the fistula, as no aperture was now left. I then found that at the right side these bands were so elongated that I could introduce my finger into the upper chamber, and distinctly feel the os uteri and surrounding parts uninjured. The parts were very moist, but no urinous smell either from the dress or my finger when withdrawn. She asked me to take her under my care in the approaching confinement, &c. However, circumstances prevented this, and she was delivered under the care of Dr. Williams, of Dingle, without difficulty, after a two hours' labour. The child was dead, and commencing to decay. All went well for two days, when diarrhœa and vomiting set in, and she finally sank on the fourteenth day."

Case VI.—An elderly woman at Parsonstown, under my friend Dr. Woods. This was a puckered cicatrix, with tough edges of long standing. I operated upon this woman by Bozeman's method, and regret to say that it appears to have totally failed. I never saw the patient except the day of the operation, but the subsequent treatment—viz., perfect repose of bowels (by opium) and of bladder by use of catheter, was fairly and fully carried out by Dr. Woods, and also the removal of the button at the end of ten days. Long before that, however, it was evident that union had failed to occur, as water made its escape per vaginam.

Case VII.—Mrs. Ryan, Limerick, sent to me by Dr. Vernon Russell, with whom I saw her in consultation before she came to town. This was a small fistula near the external parts, nearly a urethro-vaginal fistula. It was readily brought into view. The woman was in excellent health, and I thought it as fair a case as could possibly be to obtain union by the simple interrupted wire sutures. With one sweep of the knife I cut out the margin of the fistula, removing it as a perfect ring. I then inserted three sutures of iron-wire at about three lines distance from each other. The little wound lay in perfect contact and we all congratulated ourselves on the certainty of success. Great was my dismay to find on the third day that water was escaping; and, on inspection, the centre wire was found to have cut its way out like a knife, although the suture had been twisted by no means too tightly. The others followed suit, and we had the satisfaction of ultimately dealing with a fistula double the original size. As soon as parts were well healed and pliable, I operated by my own method—five sutures were required. On inspection, forty-eight hours after

operation, I found a tendency to ulcerate underneath the quills, and I divided the ligatures at one side, relaxing all, but removing nothing. Next day I found the anterior quill loose, and removed it; and the following day I removed the remaining quill and sutures. All had healed, and in a few days she was safe and able to move about. She went home perfectly well, and has so continued ever since.

Case VIII.—Mrs. Cush, of Dungannon. A formidable chasm, exposing the upper wall of the bladder and the urethra posteriorly. I had immense difficulty in bringing the sides together at all by seven sutures (quilled). I was satisfied to get three of these to hold, and so to reduce the gap one-half, or nearly so, in diameter. This woman has gone back to the country for six months, and I merely give her case lest I should be accused of omitting failures. She might, however, without straining my professional conscience, be claimed as still under treatment.

Case IX.—Mrs. Hill, Dublin, aged about 30. Small woman. Small vagina. Prolonged labour with instruments, 1854. Came to me in the latter end of 1860, with almost complete loss of the floor of the bladder. The gap admitted four fingers. It seemed a very hopeless case. I operated on her, however, and with great difficulty succeeded in bringing the two sides in fair contact by an obliquely transverse line of union. The right end of the quills was near the external parts, and the left went back more into the vagina and near the os uteri. Dr. Beatty, and several other accoucheurs, saw this operation; and the universal opinion of the bystanders was, that there was little chance of improvement. On the fourth day I had to let go several of the threads, as ulceration was imminent under the quills, from the great strain upon them. Unfortunately, I removed the remainder on the fifth day; and although the greater part of the fissure had united, yet the union was too soft to hold, and in the end, a bar of an inch and a quarter in the centre of the line of union held on, and became firmly consolidated. As a remarkable effect of the operation, I may mention that menstruation, which had ceased to manifest itself for six years, became re-established. One of my sutures had traversed the anterior lip of the os uteri, and the obliterated canal became in this way restored. The woman had suffered severely at each menstrual period, but from this out her sufferings ceased. I sent her away for some months; and on her return in February, 1861, I found the centre bar about an inch in length, shortened up, and firm. The remainder of the huge rent was thus cut up into two portions: the right was funnel-shaped, and would just admit the tip of the little finger; the

left rent was irregular, and about double this in the long diameter. I operated on the latter by my own method, and reduced it to a slit, which I purpose attacking in a few days. The former I operated on by the button method, and have closed it. Thus, in three operations this formidable chasm has been reduced to a mere slit; and the patient is herself conscious of the great improvement that has been obtained by an increase to her comfort, and by a certain though very limited amount of control over her bladder. Formerly she was wet both sitting and lying, as well as standing; now she can retain her water both when sitting and lying for some hours, and even standing, for a few minutes. Such a case as this would encourage me to operate on almost any case however apparently unpromising.

Case 10.—The last case I have to enumerate is that of Mrs. Jordan, at present under treatment for a small fistula, which I have reduced to about the size of a crow-quill, and am trying to close by cautery. I have operated on her both by my own method and by Bozeman's. My own was spoiled by the new hemp ligature, which I unfortunately used, and it cut off my flaps. I then tried Bozeman's plan, and have reduced it, as above stated.

To sum up the results of my ten cases:—I had three cured by the operation before leaving me—Cases I., III., VII., all by my own method.

Case V. all but closed on leaving hospital, was finally cured by one touch of caustic. In this the cure by my method would have been complete but for diarrhœa.

Case II. was relieved of all distressing symptoms by one operation after my method; and although a pin-hole seems to remain, she has no stillicidium, unless after a long journey.

Case IV.—Fistula reduced to a minimum by my method, but patient unrelieved.

Case VI.—Failure. Bozeman's operation.

Case VIII.—Partial closure of large chasm by my operation.

Case IX.—Under treatment. Reduced from two inches, square, to a slit of a few lines in length.

Case X.—Under treatment. Reduced one-half.

Five have been restored to comfort; one has absolutely failed by Bozeman's method; two have had their fistulas considerably reduced in size; and if they return, may derive still further benefit, and like the remaining two, still under treatment, may have a fair chance of final cured.

I shall now throw the cases into a tabular form, that the results may be seen at a glance.

| Name and Residence. | Age. | Date of Fistula. | Size of Fistula. | Probable Cause of Fistula. | Situation of Fistula. | Complications. | Primipara, or otherwise. | Date of Operation. | Nature of Operation. | Date of Removal of Apparatus. | Result as regards Fistula. | Result as regards Control over Bladder, &c. |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Giblin, Navan. | 25 | February, 1856. | 1¼ inch. in length. | Seventy hours in labour. | Neck of bladder. | . . . . . | Prim. | Mar. 31, 1856. | Collis'. | Five days. | Cure. | Cure subsisting after subsequent deliveries. |
| Balfe, County Meath. | 40 | March, 1855. | 3 inches in length. | Four days' labour, forceps. | From meatus to os tincæ. | . . . . . | Sixth child. | Sep. 24, 1856. | Collis'. | Do. | Pinhole. | Complete, except after a journey. |
| Madden, Co. Armagh. | 30 | 1855. | Irregular. 2½ by 2 in. | Crucial wound, by razor. | Floor of bladder. | . . . . . | Prim. | Feb. 4, 1857.<br>Mar. 5. 1857.<br>May, 1857.<br>June, 1857. | Collis'.<br>Collis'.<br>Bozeman's.<br>Collis's. | . . . . .<br>. . . . .<br>. . . . .<br>. . . . . | Reduced.<br>Reduced.<br>No improvement.<br>Cure. | Complete until the rupture of the cicatrix, in three weeks, from coitus. |
| Levey, Portarlington. | 30 | 1847. | 1 inch. | Violence of midwife. | Floor of bladder. | . . . . . | Prim., six children since. | Oct. 26, 1857.<br>Nov. 17, 1857.<br>Dec. 1, 1857.<br>Dec. 17, 1857. | Collis'.<br>Collis'.<br>Hare-lip needle.<br>Bozeman's. | . . . . .<br>. . . . .<br>. . . . .<br>. . . . . | Reduced to size of crowquill.<br>Size of probe.<br>Enlarged.<br>Pinhole. | No improvement; subsequent parturitions have not increased the fistula. |
| Murphy, Tralee. | . . | 1859. I believe. | 1 inch. | Prolonged labour. | Floor of bladder. | Adhesions of vagina. | Not known. | May 11, 1859.<br>June 1, 1859.<br>Sep. 1859. | Collis'.<br>Bozeman's.<br>Caustic. | 4th and 5th days.<br>10th day.<br>. . . . . | Cure, but torn by vomiting.<br>Pinhole.<br>Cure. | Complete. |
| Ryan, Limerick. | 30 | . . . . | 4 lines in diam. | Pressure of head of fœtus. | Neck of bladder. | . . . . . | . . . . . | . . . . . | Simpson's<br>Collis'. | In 3 days cut out.<br>48 hours. | Failure.<br>Cure. | Complete. |
| ——— Parsonstown. | 50 | Many years. | ½ inch. in diam. | Protracted labour. | Floor of bladder. | . . . . . | . . . . . | Sep. 1859. | Bozeman's. | 10 days. | Failure. | |
| Hill, Dublin. | 32 | 1854. | 2 inches square. | . . . . . | Floor of bladder. | Impervious canal of cervix. | Prim. | Nov. 1860.<br>Feb. 1861.<br>Mar. 1861. | Collis'.<br>Collis'.<br>Bozeman's. | 4th and 5th days.<br>Do.<br>10 days. | Centre bar of 1½ inch united.<br>Portion operated on reduced to a slit.<br>Portion operated on cured. | Great improvement.<br>Under treatment. |
| Cush, Dungannon. | . . | . . . . | Immense. | Prolonged labour. | Floor of bladder. | Adhesions of vagina. | . . . . . | . . . . . | Collis'. | 6th day. | Reduced one-third. | Gone to the country. |
| Jordan, Leixlip. | 24 | . . . . | ¼ inch. in diam. | Prolonged labour. | Floor of bladder. | . . . . . | Prim. | . . . . . | Collis'.<br>Bozeman's. | 4th day.<br>12th day. | Failure.<br>Reduced. | Under treatment. |

Art. XIII.—*On the Phenomena of Diabetes Mellitus.* By the Rev. Samuel Haughton, F. R. S., Fellow of Trinity College, Dublin.

The history of this remarkable disease affords an instructive example of the slow growth of human knowledge with regard to questions of physiology. The name diabetes (διάβήτης, a syphon) was first mentioned by Aretæus, the Cappadocian, as a title of the class of diseases in which diuresis was a prominent symptom, either constant or occasional. He appears to have included dropsy, of various kinds, under the title of diabetes; and there can be no doubt but that the disease now called diabetes mellitus would have been placed by him under the same head. His reason for the disease having been named diabetes is thus stated:—"τῇδέ μοι δοκέει καλέεσθαι διάβήτης ἐπίκλησιν ὁκοῖόν τι διαβήτης ἐὼν, ὕυνεκεν ἐν τῷ σκήνεϊ τὸ ὑγρὸν ὀυ μίμνει, ἀλλὰ ὅκως διαβάθρῃ τῷ ἀνθρώπῳ ἐς ἔξοδον χρέεται."—*Lib.* i. *chap.* 2.

Aretæus had no suspicion that sugar, or any abnormal element, was contained in the urine of diabetic sufferers; and this leading and prominent fact appears to have remained unnoticed until it attracted the attention of the learned and observant Dr. Thomas Willis, of Christ Church, Oxford, who thus states the fact[a]:—"Quod autem plerique authores potum aut parum aut nihil immutatum reddi afferunt, à vero longissime distat: quoniam urina in omnibus (quos unquam me novisse contigit, et credo ita in universis habere) tum a potu ingesto, tum à quovis humore in corpore nostro gigni solito, plurimum differens, quasi melle aut saccharo imbuta, mire dulcescebat."

It has been subsequently ascertained that the sugar of diabetic urine is invariably grape sugar ($C_{12}H_{12}O_{12} + 2HO$), which is not a natural or normal product of the urine in health; and that some of the usual natural products of the urine are present in exaggerated proportion. This is now known to be true in particular of the urea, which was formerly thought to be complementary to the sugar, to increase as the latter diminished, and *vice versâ*. In 1832, Sir Robert Kane, then Professor of Chemistry to the Apothecaries' Hall, published the results of five cases in which he had determined the amount of urea in diabetic urine[b], and expressed the opinion that the urea and sugar in diabetes were not complementary; and that in a given time there was probably as much urea excreted as in health. The following are the results he obtained:—

[a] *Vide* Lib. De Medicamentorum Operationibus, p. 64, published at Amsterdam, in 1682.

[b] Dublin Journal of Medical and Chemical Science, vol. i. p. 15.

TABLE I.—*Kane's determination of Sugar and Urea in Diabetes Mellitus.*

| Case. | Sugar in 1000 parts. | Urea in 1000 parts. | Specific gravity. |
|---|---|---|---|
| 1 | 47·0 | 9·0 | 1032·00 |
| 2 | 31·5 | 9·5 | 1030·25 |
| 3 | 60·0 | 6.5 | 1036·25 |
| 4 | 51·0 | 5.3 | 1033·00 |
| 5 | 70·0 | 13·5 | 1050·50 |

The quantity of urine per day passed by case 5, is stated as 10 pints, which would give **170** grains of urea per day. This quantity, although not equal to the average of health, is more than was supposed at the time to be excreted. It is now well known that the quantity excreted is much greater than that excreted in health. In Dr. Parkes' recent work on Urine,[a] the urea passed per day in twelve cases, given by various authorities, ranges from 421 grains to 1,411 grains, and has a mean of **811** grains per day. My own observations quite confirm these results.

Much difference of opinion exists as to the quantity of phosphoric acid passed by diabetic patients, the general impression being that it is but little altered. In this opinion I cannot coneur, as I have invariably found it greatly increased; this is shown by the following table:—

TABLE II.—*Amount of Phosphoric Acid passed in Diabetes Mellitus.*

| Case. | Phosphoric acid per day. | | Total. | Urea per day. |
|---|---|---|---|---|
| | With earths. | With alkalies. | | |
| Keogh, . . . | 76 grs. | 132 grs. | 208 grs. | 750 grs. |
| M'Nee, . . . | 24 ,, | 49 ,, | 73 ,, | 1202 ,, |
| Murphy, . . | 38 ,, | 56 ,, | 94 ,, | 1559 ,, |

If diabetes be a disease accompanied by rapid and great change of tissue, nothing can be more natural than the in-

[a] Page 341.

creased excretion of urea, phosphoric acid, chlorine, and all the other healthy constants of urine. I shall reserve, however, all speculation on the physiology of diabetes for the concluding portion of this paper, as I am anxious to keep quite distinct the facts I have observed, and the inferences I think may be drawn from them.

It is commonly supposed that the specific gravity of the urine affords a means of determining the quantity of sugar, without the trouble of chemical experiments. The extent to which this is true may be inferred from the following Table, in which I have collected together the specific gravities of the various diabetic urines, determined by myself with the specific gravity bottle, and also the quantity of sugar per fluid ounce, found by direct experiment from the same specimens of urine:—

TABLE III.—*Comparison of Specific Gravity of Diabetic Urine with the quantity of sugar per ounce*

| Specific gravity. | Sugar per ounce. | Name of patient. |
|---|---|---|
| 1007·7 | 1·2 grs. | M'Cabe. |
| 1031·8 | 33·6 ,, | Murphy. |
| 1033·1 | 27·3 ,, | M'Nee. |
| 1033·8 | 33·6 ,, | Murphy. |
| 1033·8 | 31·2 ,, | ,, |
| 1034·0 | 31·2 ,, | M'Nee. |
| 1035·0 | 40·9 ,, | Murphy. |
| 1035·4 | 36·4 ,, | M'Nee. |
| 1035·7 | 33.6 ,, | Murphy. |
| 1035·9 | 36·4 ,, | M'Nee. |
| 1038·1 | 32·0 ,, | Keogh. |
| 1038·3 | 39·8 ,, | Murphy. |
| 1040·1 | 39·8 ,, | ,, |
| 1040·2 | 39·7 ,, | ,, |
| 1040·6 | 39·7 ,, | ,, |

From this Table it appears that if only an approximation be required, for specific gravities between 1030 and 1040, there is no great error in assuming the excess above 1000, as the number of grains of sugar in each ounce of diabetic urine.

In the following cases of diabetes, I endeavoured to ascertain carefully the composition of the food consumed, with regard to its power of producing urea and glucose, so as to be

able to compare the food ingested with the excretions. In order to ascertain the composition of the food, I made the following experiments, in addition to those detailed in my paper on the "Natural Constants of the Healthy Urine of Man"[a].

*Additional Experiments on Food.*

1. *Brown Bread*, used in the Meath Hospital.
   200 grs., dried at 212° F., gave 155·00 grs.
   26·4 grs. of these gave 4·26 grs. of platinum.
   Solids = 77·50 per cent.
   Nitrogen = 2·30 ,,

From these data, I find the following chain, to determine the quantity of urea due to 1 lb. of this bread:—

1 lb. brown bread.
1 — 7000 grs. ,,
1000 — 775 grs. at 212° F.
1000 — 23 grs. nitrogen.
28 — 60 grs. urea.

**267** grs. urea.

This is a much larger proportion of urea than I found producible from white bread, even of the best quality,—1 lb. of which only produces 196 grs. of urea.

2. *Rice*, used in the Meath Hospital:—
   100 grs., dried at 212° F., gave 85·3 grs. solids.
   23·0 grs. of these gave 3·10 grs. of platinum.
   From which I deduce:—
   Solids = 85·30 per cent.
   Nitrogen = 1·92 ,,

The chain to determine the urea producible from 1 lb. of this rice is the following:—

1 lb. rice.
1 — 7000 grs.
1000 — 853 grs. at 212° F.
10,000 — 192 grs nitrogen.
28 — 60 grs. urea.

**245** grs. urea.

3. *Porter.* For the following analysis of two kinds of Dublin porter, I am indebted to Dr. Apjohn:—
   A. 1000 grs., dried at 212° F., gave 57.9 grs. of solids. 35·6 grs. of these, burned with soda lime, gave 3·40 grs. of sal ammoniac.
   B. 1000 grs., dried at 212° F., gave 84·8 grs. of solids. 35·4 grs. of these gave 2·30 grs. of sal ammoniac.

[a] Dublin Quarterly Journal of Medical Science, vol. xxx., pp. 2, 8.

From these experiments, I find :—

A.

1 quart porter.
4 — 70000 grs. ,,
10,000 — 579 grs. at 212° F.
356 — 34 grs. sal ammon.
54 — 14 grs. nitrogen.
28 — 60 grs. urea.

**53·76** grs. urea.

B.

1 quart porter.
4 — 70000 grs.
10,000 — 848 grs. at 212 F.
354 — 23 grs. sal ammon.
54 — 14 grs. nitrogen.
28 — 60 grs. urea.

**53·57** grs. urea.

The agreement between these two results is remarkable, and proves that in good Dublin porter the quantity of nitrogen is independent of the solids present, which may vary considerably according to the proportion of the other constituents present.

4. *Tea*, used in the Meath Hospital. The tea given to the patients in this hospital is made in the proportion of one drachm to the pint, or one ounce to the gallon. On analyzing this tea leaf in the usual manner, I found—

70·6 grs., dried at 212° F., gave 60 grs. of solids.
26·3 grs. of these gave 8·70 grs. of platinum.

Solids = 85·00 per cent.
Nitrogen = 4·73 ,,

From these data, I deduce the following chain :—

1 lb. tea.
1 — 7000 grs.
100 — 85 grs. at 212° F,
10,000 — 473 grs. nitrogen.
28 — 60 grs. urea.

**603** grs. urea.

5. *Boiled Cabbage*, used in Meath Hospital.

965 grs., dried at 212° F., gave 86·6 grs. of solids.
28·2 grs. of these gave 8·35 grs. of platinum.

Solids = 8·97 per cent.
Nitrogen = 4·23 ,,

From these data, the following chain is deducible :—

1 lb. cabbage (boiled).
1 — 7000 grs. ,,
10,000 — 897 grs. at 212° F.
10,000 — 423 grs. nitrogen.
28 — 60 grs. urea.

**57** grs. urea.

The quantity of glucose sugar producible from the various kinds of food used, may be thus estimated:—

1. *Brown Wheaten Bread.* The quantity of urea due to a pound of this bread has been already determined by direct experiment, p. 320; and the sugar producible from it may be found from the following considerations:—

I. According to Peligot's researches, it appears that in whole wheaten meal, on the average, the proportion of proteinic to starch compounds is as 14 per cent. to 68 per cent.

II. The proteinic compounds are to urea as 3 to 1.

III. Starch will produce glucose, in the proportion of 81 to 99.

From these data the subjoined chain follows:—

1 lb. brown bread.
1 — 267 grs. urea.
1 — 3 grs. proteinic compounds.
14 — 68 grs. starch.
81 — 99 grs. glucose.

**4755** grs. glucose.

Hence 2 lbs. of this bread are equivalent to 9510 grains of glucose.

2. *Rice.* According to Boussingault, rice contains 83 portions of starch to 7·5 of azotised matter; combining this with the result obtained by myself, I find:—

1 lb. rice.
1 — 245 grs. urea.
1 — 3 grs. azotised matter.
75 — 830 grs. starch.
81 — 99 grs. glucose.

**9942** grs. glucose.

Hence 2 ounces of rice are equivalent to 1243 grains of glucose.

3. *Milk.* I have found that the Dublin milk contains lactose, equivalent to glucose, in the proportion of 177 grains to the pint. Therefore two quarts of milk are equal to 708 grains of glucose.

4. *Porter.* It has been found that 1000 grains of Dublin porter contains three grains of glucose sugar.[a] Hence the sugar in one quart of porter is thus found:—

1 quart porter.
4 — 70,000 grs. „
1000 — 3 grs. glucose.

**52·5** grs. glucose.

[a] Dublin Quarterly Journal of Science, Vol. I. No. 2.

Combining together the results of all my experiments, I have constructed the following table:—

TABLE IV.—*The Urea and Sugar Equivalents of several kinds of human Food.*

| No. | Food. | Quantity. | Urea Equivalent. | Sugar Equivalent. |
|---|---|---|---|---|
| 1 | *Lean roasted mutton;* leg, near shank end; contains fat and fascia . . . . . | 1 lb. | 480 grs. | |
| 2 | *Lean roasted mutton;* leg, near the loin; very little fat or fascia . . | 1 lb. | 957 „ | |
| 3 | *Lean raw mutton,* loin . . . . | 1 lb. | 544 „ | |
| 4 | *Lean roasted beef,* sirloin . . . | 1 lb. | 724 „ | |
| 5 | *Lean raw beef,* sirloin . . . . | 1 lb. | 979 „ | |
| 6 | White bread, first quality . . . | 1 lb. | 196 „ | |
| 7 | White bread, second quality . | 1 lb. | 203 „ | |
| 8 | Brown bread, wheaten . . . | 1 lb. | 267 „ | 4,755 grs. |
| 9 | Oatmeal . . . | 1 lb. | 421 „ | |
| 10 | Indian meal . . | 1 lb. | 150 „ | |
| 11 | Rice . . . . | 1 lb. | 245 „ | 9,942 grs. |
| 12 | Cauliflower, boiled . . . | 1 lb. | 58 „ | |
| 13 | Cabbage, boiled . | 1 lb. | 57 „ | |
| 14 | Tea . . . . . | 1 lb. | 603 „ | |
| 15 | Porter . . . . | 1 qt. | 54 „ | |
| 16 | Milk, sp. gr.=1025 | 1 qt. | 116 „ | 354 grs. |
| 17 | Milk, sp. gr.=1027 | 1 qt. | 126 „ | |
| 18 | Milk, sp. gr.=1035 | 1 qt. | 174 „ | |

From this table it appears that one pound weight of lean beef or mutton is capable of producing from 500 to 1000 grains of urea, according as its muscular fibre is more or less mixed with cellular fat. The highly nutritious qualities of oatmeal,

wheaten bread, and rice, as compared with Indian meal, are also apparent; and the agreement between the cabbage and cauliflower is natural.

The first case of diabetes mellitus that I shall describe is that of Owen Murphy; for the opportunity of observing this case I am indebted to the kindness of Dr. Stokes, under whose care he was in the Meath Hospital. The history of his case was taken by Mr. A. W. Foot, one of Dr. Stokes' pupils, whose zeal and intelligence are highly to be commended. I have added to my own remarks the statement of Mr. Foot, which renders any reference to details useless on my part.

Owen Murphy came under my observation in the beginning of November, 1860, and died on the 14th January, 1861. Tubercle was first detected in his right lung on the 27th November, 1860.

The following Table contains an account of his weight (naked), urine, its specific gravity, and the quantity of urea and sugar it contained; the specific gravity of the urine, its sugar, and urea, were observed on a mixture of all the urine passed in the 24 hours:—

Table V.—*Constants of Body and Urine of Owen Murphy*, 1860–61.

| Date. | Weight. | Urine. | Sugar per day. | Urea per day. | Specific gravity. |
|---|---|---|---|---|---|
| 1860, 8th Nov. | 98 lbs. | 310 ℥ fl. | 12,329 grs. | 1559 grs. | 1040·2 |
| „ 15th „ | 92·5 „ | 363[a] „ | 12,216 „ | 1429 „ | 1033·8 |
| „ 22nd „ | 93·5 „ | 360 „ | 11,250 „ | 1420 „ | 1033·8 |
| „ 29th „ | 95·5 „ | 300 „ | 12,290 „ | 1181 „ | 1035·0 |
| „ 6th Dec. | 96·5 „ | 275 „ | 9255 „ | 1203 „ | 1035·7 |
| „ 13th „ | — | 240 „ | 9545 „ | 1181 „ | 1040·6 |
| „ 20th „ | 93·5 „ | 175 „ | 6960 „ | 976 „ | 1038·3 |
| 1861, 3rd Jan. | 91·5 „ | 194 „ | 7716 „ | 849 „ | 1040·1 |
| „ 10th „ | 87·5 „ | 190 „ | 6394 „ | 840 „ | 1031·8[b] |
| Mean, . . . | **93·56** | **267·4** | | | |

[a] Gives off a vinous smell, and carbonic acid gas.

[b] This sp. gr. is not to be depended on, as carbonic acid gas was being rapidly evolved from the fermenting urine during the weighing.

The quantities of urea and sugar ingested are found from the following calculations, based on the quantities of food and its composition already determined:—

UREA EQUIVALENT OF FOOD.

1.—*Food per day previous to 8th November*, 1860.

| | | | |
|---|---|---|---|
| 1. Brown bread, 2 lbs., | equivalent to | 534 | grs. urea. |
| 2. Beef, $\frac{2}{7}$ths of lb. | ,, | 207 | ,, |
| 3. Rice, 2 oz., | ,, | 31 | ,, |
| 4. Milk, 2 quarts, | ,, | 232 | ,, |
| 5. Porter, 1 quart, | ,, | 54 | ,, |
| 6. Tea, 1 pint, | ,, | 5 | ,, |
| 7. Eggs, 2, | ,, | 90 | ,, |
| | | **1153** | grs. |

2.—*Food per day previous to 15th November*, 1860.

Same as No. 1. Food changed to following on 16th November.

3.—*Food per day previous to 22nd November*, 1860.

| | | |
|---|---|---|
| 1. Brown bread (toasted), 2 lbs., | 534 | grs. urea. |
| 2. Beef, 1 lb. (boiled), | 724 | ,, |
| 3. Rice, 2 oz., | 31 | ,, |
| 4. Milk 2 quarts, | 232 | ,, |
| 5. Porter, 1 quart, | 54 | ,, |
| 6. Tea, 1 pint, | 5 | ,, |
| 7. Eggs, 2, | 90 | ,, |
| | **1670** | grs. urea. |

4.—*Food per day previous to 29th November*, 1860.

| | | |
|---|---|---|
| 1—7. Same as before, . . . | 1670 | grs. urea. |
| 8. Boiled cabbage, 2 lbs. . | 114 | ,, |
| | **1784** | grs. |

5. *Food previous to 6th December*, 1860.

Same as last, with a diminution of the cabbage to 1 lb. daily. This gives for the daily equivalent of urea—

**1727** grs. urea.

6. *Food previous to 13th December*, 1860.

Same as last, with reduction of bread to 1 lb. daily. This gives for daily equivalent of urea— **1460** grs. urea.

7. *Food previous to 20th December*, 1860.

Same as last; but on 18th December he complained that he had no appetite nor thirst.

8. *Food previous to 3rd January*, 1861.

Meat reduced to ½ lb. daily; milk reduced to 1 quart; and cabbage altogether omitted. This gives for equivalent of urea—

| | | |
|---|---|---|
| 1. Bread, 1 lb., | 267 | grs. urea. |
| 2. Beef, ½ lb., | 362 | ,, ,, |
| 3. Rice, 2 oz., | 31 | ,, ,, |
| 4. Milk, 1 quart, | 116 | ,, ,, |
| 5. Porter, 1 quart, | 54 | ,, ,, |
| 6. Tea, 1 pint, | 5 | ,, ,, |
| 7. Eggs, 2, | 90 | ,, ,, |
| | **925** | grs. urea. |

9. *Food previous to 10th January*, 1861.

Same as last, with omission of quart of milk.
This is equivalent to **809** grs. urea.

Sugar Equivalent of Food.

1. *Food previous to 8th November*, 1860.

| | | |
|---|---|---|
| 1. Brown Bread, 2 lbs., | 9510 | grs. sugar. |
| 2. Rice, 2 oz., | 1243 | ,, ,, |
| 3. Milk, 2 quarts, | 708 | ,, ,, |
| 4. Porter, 1 quart, | 52 | ,, ,, |
| | **11,513** | grs. sugar. |

2. *Food previous to 13th December*, 1860.

The sugar-forming elements of the food remained unaltered up to the 6th December, when the bread was reduced to 1 lb. daily. Hence—

| | | |
|---|---|---|
| 1. Brown bread, 1 lb., | 4,755 | grs. sugar. |
| 2. Rice, 2 oz., | 1,243 | ,, ,, |
| 3. Milk, 2 quarts, | 708 | ,, ,, |
| 4. Porter, 1 quart, | 52 | ,, ,, |
| | **6758** | grs. sugar |

3. *Food previous to 3rd January*, 1861.

| | | |
|---|---|---|
| 1. Brown bread, 1 lb. . . . | 4755 grs. | sugar. |
| 2. Rice, 2 oz., . . . . . | 1243 | ,, |
| 3. Milk, 1 quart, . . . . | 354 | ,, |
| 4. Porter, 1 quart, . . . | 52 | ,, |
| | **6404** grs. | sugar. |

The following Table gives a summary of the preceding calculations, and a comparison of their results, with the observations on the urine.

TABLE VI.—*Showing the Quantities of Urea and of Sugar ingested and excreted per Day by Owen Murphy*, 1860–61.

| Date. | Urea ingested. | Urea excreted. | Sugar ingested. | Sugar excreted. |
|---|---|---|---|---|
| 1860, 8th Nov. | 1153 grs. | 1559 grs. | 11,513 grs. | 12,329 grs. |
| ,, 15th ,, | 1153 ,, | 1429 ,, | 11,513 ,, | 12,216 ,, |
| ,, 22nd ,, | 1670 ,, | 1420 ,, | 11,513 ,, | 11,250 ,, |
| ,, 29th ,, | 1784 ,, | 1181 ,, | 11,513 ,, | 12,290 ,, |
| ,, 6th Dec. | 1727 ,, | 1203 ,, | 11,513 ,, | 9255 ,, |
| ,, 13th ,, | 1460 ,, | 1181 ,, | 6758 ,, | 9545 ,, |
| ,, 20th ,, | 1460 ,, | 976 ,, | 6758 ,, | 6960 ,, |
| 1861, 3rd Jan. | 925 ,, | 849 ,, | 6404 ,, | 7716 ,, |
| ,, 10th ,, | 809 ,, | 840 ,, | 6404 ,, | 6394 ,, |

Reducing the results of the preceding Table to grs. per pound of body-weight, I obtain the two following Tables:—

TABLE VII.—*Showing the Relation of the Sugar ingested to the Sugar excreted, per Day, per Pound of Body-weight.*

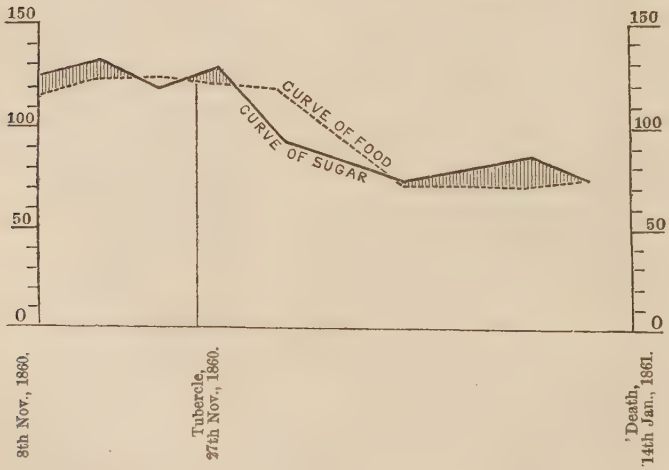
| Date. | Sugar ingested, in grains, per pound of body-weight. | Sugar excreted, in grains, per pound of body-weight. |
|---|---|---|
| 1860, 8th November, | 117 grs. | 126 grs. |
| ,, 15th ,, | 124 ,, | 132 ,, |
| ,, 22nd ,, | 123 ,, | 120 ,, |
| ,, 29th ,, | 120 ,, | 129 ,, |
| ,, 6th December, | 119 ,, | 96 ,, |
| ,, 20th ,, | 72 ,, | 74 ,, |
| 1861, 3rd January, | 70 ,, | 84 ,, |
| ,, 10th ,, | 73 ,, | 73 ,, |

Table VIII.—*Showing the Relation of the Urea ingested to the Urea excreted, per Day, per Pound of Body-weight.*

| Date. | Urea ingested in grains per pound of body-weight, | Urea passed, per vesicam, in grains per pound of body weight. |
|---|---|---|
| 1860, 8th November, | 11·8 grs. | 15·9 grs. |
| ,, 15th ,, | 12·5 ,, | 15·4 ,, |
| ,, 22nd ,, | 17·8 ,, | 15·2 ,, |
| ,, 29th ,, | 18·7 ,, | 12·3 ,, |
| ,, 6th December, | 17·9 ,, | 12·4 ,, |
| ,, 20th ,, | 15·6 ,, | 10·4 ,, |
| 1861, 3rd January, | 10·1 ,, | 9·3 ,, |
| ,, 10th ,, | 9·2 ,, | 9·6 ,, |

The two preceding Tables are graphically exhibited in the following diagrams:—

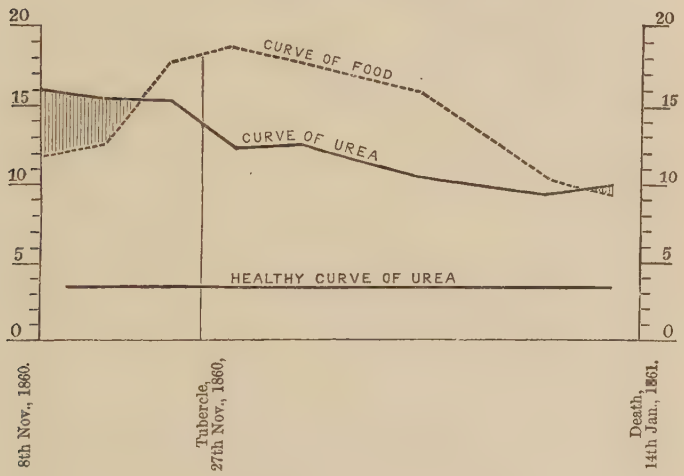
No. 1.—*Sugar excreted and ingested.*




In this diagram the horizontal line represents the time from 8th November, 1860, to Murphy's death, 14th January, 1861. The vertical ordinates are drawn to scale to represent grains of sugar per pound weight of body. The full line, called "curve of

sugar," represents the sugar excreted in the urine; and the dotted line, called "curve of food," represents the equivalent of sugar ingested, calculated by converting the starch, &c., into glucose. I have shaded the intervals between the curves, when the sugar excreted exceeded the sugar ingested; during this portion of time the disease must have converted the body-tissues into glucose.

No. 2.—*Urea excreted and ingested.*




In this diagram the horizontal line is the same as before, and the vertical ordinates, drawn to scale, represent grains of urea per pound weight of body. The full and dotted lines represent the *excreta* and *ingesta;* and I have shaded their intervals, when the urea excreted exceeded the urea ingested; where this shaded space occurs the urea excreted must have been produced partly by the destruction of the proteinic tissues of the body. I have placed on the same diagram the horizontal line of 3·5 grs. urea per lb. of body-weight, in order to show how excessive the excretion of urea is in diabetes mellitus.

The following Table shows the relation of the discharge of sugar and urea to the body-weight:—

Table IX.—*Ratio of Excretion of Sugar to Excretion of Urea.*

| Date. | Grains of Sugar per day, per pound, of body-weight. | Grains of urea per day, per pound, of body-weight. | Ratio of Sugar to urea. |
|---|---|---|---|
| 1860, 8th Nov. | 126 grs. per lb. | 15·9 grs. per lb. | 7·9 |
| ,, 15th ,, | 132 ,, | 15·4 ,, | 8·5 |
| ,, 22nd ,, | 120 ,, | 15·2 ,, | 7·8 |
| ,, 29th ,, | 129 ,, | 12·3 ,, | 10·4 |
| ,, 6th Dec. | 96 ,, | 12·4 ,, | 7·7 |
| ,, 20th ,, | 74 ,, | 10·4 ,, | 7·1 |
| 1861, 3rd Jan. | 84 ,, | 9·3 ,, | 9·0 |
| ,, 10th ,, | 73 ,, | 9·6 ,, | 7·6 |
| Mean, . . . . . | | | **8·25** |

The following is Mr. A. W. Foot's account of the case, which was carefully observed by him from day to day:—

"Owen Murphy, aged 20, from Balbriggan, was admitted into the Meath Hospital, October 10th, 1860.

"*Previous History.*—He was in good health fourteen months before admission. In the month of November, 1859, he got a fever, in the course of which he had vomiting, diarrhœa, and an eruption on his body. He was in Ardee Hospital for two weeks; he left this hospital on his convalescence, because he he did not get enough to eat. A month after doing so, he began to suffer from excessive thirst; and in two or three weeks more, from great appetite; at the same time he commenced to make water in large quantity, of a "sea-green colour, and having the smell of apples." He had got his livelihood by making bricks in summer, and by out-door farm-work in winter.

"*Condition when admitted.*—Thirst and appetite excessive, passing daily large quantities of pale urine, full of sugar, and of a high specific gravity. Skin dry; never moist. Bowels habitually confined. Muscles thin and weak. Face clay-coloured; and countenance dejected and cold-looking. Sight defective; has been so since commencement of present illness; was good before; the pupils are dilated, and contract sluggishly to light; he sees best at noon; sometime has double vision; sees sparks of light when the eyes are long directed

upon one object. Has a constant heat in his body; so that he throws off his bed-clothes, even on cold nights, to get cool. Cold water relieves this feeling of warmth best. Prepuce long, and cannot be retracted. Pulse rapid (100). Tongue red at the tip, covered towards its back part, with soft brown paste. Suffers often from giddiness.

"*Treatment and Progress.* October 14th. Recipe:—Of compound rhubarb pill, a drachm and half; of creasote, ten drops, made into eight pills, one three times daily; and an ounce of lime water in an ounce of milk every third hour.

"Ordered brown bread, and to have it toasted.

"For the first four or five days which he was in hospital he was on milk diet, and consequently said he could not remain, his hunger was so great. He had previously left two other hospitals in Dublin, because he did not get enough to eat. He used to support himself at home by begging among the farmers' houses; and stated that he generally ate 4 or 5 lbs. of meat and as much bread, daily. He expressed a liking for fat meat, and an especial preference for roasted fat. He found his thirst assuaged best by porter; buttermilk, which he sometimes drank to distension, increased his thirst. He was soon ordered full diet: a pint of porter daily, two eggs, an additional quart of milk, and rice for supper instead of stirabout, the usual food.

"October 26th. Ordered a teaspoonful of rennet in water three times a day. This he took till November 22nd.

"Thursday, Nov. 8th. Weighed him; bladder empty. Deducting the weight of his clothes, he weighed seven stone.

"The diet he was taking at this time was the following:—

"*Breakfast.*—Brown bread, 1 lb.; milk, 1 quart; eggs, 2.

"*Dinner.*—Beef, boiled, ½ lb. four days in week; brown bread, 1 lb.; porter, 1 quart.

"*Supper.*—Rice, 2 oz.; milk, 1 quart; tea, 1 pint.

"The quantity of urine excreted from 9 A. M. on the 9th, to the same hour on the 10th of November, was 15½ pints; colour, light golden; specific gravity, 1040·2; containing sugar, 12329 grs.; urea, 1559 grs.; the grains of urea per lb. weight being 15·9.

"Thursday, November 15th. His naked weight was 6 st. 8½ lb., showing a loss of 5½ lbs. in the preceding week. Professor Haughton accounted for this loss by the fact which he ascertained, that he was excreting more proteinic and farinaceous compounds per urinam than he was supplied with in the ingesta. The boy himself, at this time, said he had a particular craving for meat; his allowance was at once

doubled, and given every day in the week instead of but four times.

"The quantity of urine passed in twenty-four hours, on the 14th and 15th of November, was 18 pints 3 oz., muddy, and fermented vigorously; specific gravity, 1033·8; containing sugar, 12215 grs.; urea, 1429 grs; the grains of urea per lb. weight being 15·4.

"Thursday, Nov. 22nd. The creasote pills omitted. Recipe:—Of extract of aloes and extract of rhubarb, each one drachm; of sulphate of quinine, gum mastich, and pepper, each two grains; make twelve pills; one to be taken daily.

"His naked weight was 6 st. 9½ lbs. The quantity of urine passed in the previous 24 hours was 18 pints; of specific gravity, 1033·8; containing sugar, 11250 grs.; urea, 1420 grs.; the grains of urea per lb. being 15·1. An addition was made to his diet on this day; he was ordered 2 lbs. of fresh cabbage, and the juice of three lemons, daily. His appearance has improved; the clay-colour of his complexion has disappeared.

"Tuesday, Nov. 27th. A bubble was heard in the right infra-clavicular region; the right clavicle is dull on percussion; *bruit de pot felée* very distinct over second rib.

"Thursday, Nov. 29th. His naked weight, 6 st. 11½ lb.; the quantity of urine passed in the previous 24 hours was 15 pints, spec. grav. 1035·0, containing sugar 12,290 grs., urea 1181 grs., the grains per pound being 12·3.

"The diet of the preceding week was—

"*Breakfast.*—Brown bread, 1 lb.; milk, 1 quart; juice of one lemon.

"*Dinner.*—Beef, boiled, 1 lb., every day in week; brown bread, 1 lb.; porter, 1 quart; juice of one lemon.

"*Supper.*—Rice, 2 oz.; 1 quart of milk; juice of one lemon.

"Saturday, December 1st. The infra-clavicular bubble increasing in distinctness; *bruit de pot felée* well marked; is not able to eat more than half his allowance of cabbage; reduced it to 1 lb. per diem; respiration more feeble in right lung posteriorly than in left; both clavicles becoming prominent; has a dry cough, which gives him pain in the right side; heart's action and sounds normal; pulse, in sitting posture, 112.

"Thursday, December 6th. Naked weight, 6st. 12½lb.; quantity of urine in previous twenty-four hours, 13 pints 15 oz.; specific gravity, 1035.7, containing sugar, 9255 grs.; urea, 1203 grs.; the grains of urea per lb. weight being 12.4.

"Expectoration scanty, thick, yellowish; sputa described as 'sweet, like honey;' does not sleep well; a weakness comes over him at night.

"December 8th. Ordered wine 4 oz; increased next day to 6 oz.

"December 10th. Expectoration profuse, viscid, brownish; pain on coughing; gets no sleep from the perpetual coughing at night; *râles* becoming developed downwards over right side of chest.

"December 13th. Could not be weighed, as he was not able to leave his bed; the urine of 24 hours was 12 pints, spec. grav. 1040·6, containing sugar 9545 grs., urea, 1181 grs.; he can only lie on the right side; has not rested the last two nights from constant coughing; *gargouillement* very decided in right infra-clavicular region; is not able for his full allowance of diet; 1 lb of bread taken off, and the second pound of cabbage; but this latter he took again in a few days, when he was given some butter with it. Recipe:—Of prussic acid, one drop; of solution of muriate of morphia, eight drops; draught to be taken three times daily.

"December 15th. Coughs less, sleeps better, diet now is—

"*Breakfast.*—Brown bread, ½ lb.; milk, 1 quart; 2 eggs; juice of one lemon.

"*Dinner.*—Boiled beef, 1 lb.; cabbage, 1 lb.; bread, ½ lb.; porter, 1 quart; wine, 6 oz.; juice of one lemon.

"*Supper.*—Rice, 2 oz.; milk, 1 quart; juice of one lemon.

"December 18th. No appetite or thirst; he drank nothing all last night; made very little water; cough incessant; difficult to expectorate; tongue dry and brown; pulse 106; loud *râles* over right lung anteriorly; respiration feeble posteriorly; ordered flannel vest, jacket, and drawers.

"December 20th. Weighed, naked, 6 st. 8½ lb.; quantity of urine in 24 hours, 8 pints 15 oz., spec. grav. 1038·3, containing sugar 6960, urea 976; he cannot lie now off the right side; small streak of florid blood in expectoration last night.

"December 21st. Prepuce red, swelled, fissured; no appetite at all; thirst much less.

"December 31st. Asked for roast meat; allowed ½ lb. roast mutton daily; vegetables omitted, he could not eat them; a pint of tea instead of the morning milk.

"January 3rd. Weight, naked, 6 st. 7½ lb; urine of 24 hours 9 pints 14 oz., spec. grav. 1040·1, containing sugar, 7716 grs.; urea, 849 grs.

"January 8th. Thirst greater, appetite less; gums soft; bowels free; distinct crepitus along front of right lung; sometimes perspires about the head at night; decubitus on the right side.

"January 10th. Weighed 6 st. 3½ lb.; urine of 24 hours,

9 pints 10 oz.; specific gravity, 1031·8 containing sugar, 6394 grs.; urea, 840 grs.; in very low state—depressed; no appetite at all; found sugar on a qualitative examination of the expectoration; thirst excessive.

"Saturday, Jan. 12th. Began to vomit; rejected all his food through the day; continued to do so next day and Monday. On Monday, Jan. 14th, tongue dry and brown; pulse, 120, steady; headache, pain in back, depressed, and moaning; decubitus on right side; no food remained on his stomach since Saturday; his prepuce sore; had been several times syringed during the last fortnight; drank quantities of cold water all this day; mind wandered before death; his ravings were about eating; and he spoke of the quantities of fat, "roasted fat," he would eat to-morrow. Died at midnight, Monday night, quietly, without convulsions.

"*Post-mortem nine hours after death.*

"Body warm, soft, very white, shrunken.

"Brain, weight, 49 oz., healthy appearance.

"Pleuræ, no fluid. Right lung adherent to ribs, softened; tubercles thickly disseminated through it. Cavity in upper lobe, size of a walnut. Left lung, no adhesions. Cavity, 3 inches long, 1 inch wide, in upper-third; tubercles through it, more abundant towards apex.

"Heart small.

"Liver large; weight, 61½ oz.; natural colour.

"Right Kidney much larger than left; weight, 7½ oz.

"Left kidney, weight, 6 oz.

"Supra-renal glands well marked.

"Bladder thickened, large; contained about 5 oz. urine. Kept for examination. The stomach was empty, containing only a little dark-coloured fluid. The mucous membrane was thick, soft, and greyish in colour. The seminal vesicles contained abundance of dead spermatozoa."—(*A. W. Foot.*)

I examined the urine found in the bladder after death, and found it to contain mucus, and a small quantity of albumen; separating these, I examined for sugar, and found it to contain 19·29 grs. per thousand. This would only give 1519 grs. in 9 pints; which would indicate a reduction of the sugar-forming faculty to one-fourth of what it had four days previously.

I treated 245·4 grs. of blood from the heart's right ventricle, liver, and general system, by drying and percolation with alcohol (sp. gr. 0·828), and failed to detect the slightest trace of sugar.

The liver weighed 61½ oz. I treated 451 grs. of this liver

with alcohol as before, and failed to find any trace of sugar in it.

In 348·2 grs. of the fatty liver of a phthisical patient, who had died shortly before in the Meath Hospital, I had found, without any difficulty, by percolation with ether and alcohol, sugar enough to saturate 29 measures of the standard copper solution (100 measures = 5 grs. glucose), which corresponds with 4·17 grs. per thousand in the raw liver.

In Cl. Bernard's accounts of his examination of human livers for sugar, I can only find three quantitative results, all of which are considerably greater than the preceding. His results are as follows:—

TABLE X.—*Determination of Sugar in Human Livers, by Claude Bernard.*

| Case. | Death. | Name and age. | Weight of liver. | Sugar per thousand. |
|---|---|---|---|---|
| 1 | Beheaded. | Ayme, 42. | 1·300 kilogr. | 17·90. |
| 2 | ,, | Bixner, 47. | 1·300 ,, | Much. |
| 3 | ,, | Lafourcade. | 1·175 ,, | Sugar. |
| 4 | ,, | Viou, 22. | 1·200 ,, | 21·42. |
| 5 | ,, | Courtin. | 1·175 ,, | Sugar. |
| 6 | Gun-shot wound. | ——, 30. | 1·575 ,, | 11·00. |

If, as some physiologists assert, the kidney is only a filter, and its function be simply to separate the urea and sugar previously formed and circulating in the blood, there ought to be a certain proportion between the dilution of sugar in the blood and in the urine, and we should expect to find sugar in the blood in that proportion as compared with the sugar in the urine.

I have attempted to determine the proportion between the sugar in the blood and in the urine of Owen Murphy in the following manner:—

1. I assume, on the authority of Weber and Schwann, that the blood in the human body is one-eighth the weight of the body.

2. That the left ventricle holds 3 ounces.

3. That Murphy's average pulse was 100 per minute; and that his weight was 96 lbs.

4. I examined, and carefully measured the diameters of the aorta and renal arteries of a subject 82 lbs. weight, and found the following results:—

Diameter of aorta, . . . . . $1\frac{25}{160}$ of inch.
Diameter of right renal artery, . . $\frac{3}{16}$ ,,
Diameter of left renal artery, . . . $\frac{33}{160}$ ,,

I assume, for the moment, that the quantity of blood flowing through the renal artery is to the quantity flowing through the aorta in the proportion of the square of the diameter of the renal artery to the square of the diameter of the aorta. In reality much less blood than is given by this proportion flows through the renal artery, in consequence of friction; but the preceding proportion will give me a maximum result.

I find from the preceding assumptions, that in 64 beats of the heart the blood circulation of the heart was completed; that is, in 0·64 of a minute—and since the area of the aorta is to the joint areas of the renal arteries as 16·214 to 1—very nearly 12 ounces of blood passed through the kidneys in 0·64 of a minute. Increasing this to the length of the day, I obtain finally the result that **1350** pints of blood passed through Murphy's kidneys each day. This result, it must be observed, is a *maximum*; and the less it is, the stronger my argument to be founded on it becomes. The average quantity of urine passed per day by Murphy was **13·37** pints; which is almost exactly the hundredth part of the blood from which it was secreted.

In the urine found in the bladder after death, I detected 19·29 parts of sugar per thousand; this, if it were secreted previously, and pre-existed in the blood, the kidneys only acting as a filter, would correspond to 0·1929 parts of sugar per thousand in the blood. This quantity of sugar could not have escaped the tests I applied. The copper solution I used was of the following strength:—100 measures=5 grs. glucose; and a quantity of oxide of copper less than that contained in one measure may be detected as a precipitate. The following chain shows the number of measures corresponding to the quantity of blood I examined:—

1000 — 245·4 grs. of Murphy's blood.
1000 — 0·1929 grs. glucose by filter-theory.
5 — 100 measures of standard copper solution.

**0·946** measures.

This quantity of the standard solution, if decomposed by the sugar, would have been detected by me; and as there was

not the slightest trace of oxide of copper precipitate, I believe that the blood, if it contained any sugar at all, did not contain one hundredth part as much as I found in the urine.

But, in reality, the blood should have contained, if the filter-theory be true, more than double the quantity of sugar just calculated; for it is a well known principle in hydraulics, that the discharge of liquids flowing through circular pipes of different bores, varies as the *square root of the fifth power of the diameter*, and not as the *square of the diameter*, which I have assumed in the preceding calculation. If this law be used, the amount of blood passing through the aorta in a given time will be 77·384 times the amount passing through one renal artery; or 38·692 times that passing through the renal arteries conjointly, instead of 16.214 times. From this it follows that only 566 pints of blood passed through Murphy's kidneys per day, instead of 1350 pints. This would give a dilution of only $\frac{1}{42}$nd instead of $\frac{1}{100}$th in estimating the sugar in the blood as compared with that in the urine—this would correspond to two and a quarter measures of the standard copper solution, an amount, which, if reduced by the presence of sugar, would attract the notice of even an inattentive observer.

The quantity of sugar in the urine during life was much greater than that found after death; for example,—on the 29th November, in 24 hours, in 300 oz. of urine there were 12290 grs. sugar, which correspond to 93·6 grs. to 1000 grs. The corresponding quantity of sugar in the blood would be 2· 28 grs. to 1000 grs. of blood. I had no opportunity of examining the blood for sugar during life, and cannot, therefore, state what quantity existed in it.

Kane failed to detect sugar in the blood of his case No. 5, although he used a process capable of discovering 5 grs. in 1 lb. of blood; i. e., 0·7143 grs. in 1000. In this case the urine contained 70 parts of sugar in 1000; which, assuming $\frac{1}{30}$th as the dilution, would give 1·4 grs. of sugar to 1000 grs. of blood, or double that which he could have easily detected. This experiment of Sir Robert Kane, made without any theoretical view, confirms the fact I have found true in the case of Owen Murphy, in whose blood I should have found sugar, had the filter-theory been true.

The only case in which I can find a quantitative comparison between the sugar in the blood and in the urine, is one recorded by Simon (Animal Chemistry, &c., Sydenham Society Publications, vol. i. p. 327, and vol. ii. p. 295). In this case he found 86·3 parts of sugar in 1000 parts of urine, and 2·5 parts of sugar in 1000 parts of blood [the 2·5 parts not being

altogether pure, but containing extractive matter, and some salts]. This result would certainly fall in with the filter-theory, as it would seem to give a dilution of sugar in the blood not very far from $\frac{1}{42}$nd which I have found to be very near the theoretical dilution in the case of Murphy. Many circumstances, however, might combine to render the fraction of dilution greater than $\frac{1}{42}$nd, such as the passing of a larger quantity of urine; while, on the other hand, a greater weight of body, and therefore of blood, might tend to diminish this fraction. Without a knowledge of all these circumstances in Simon's and Kane's cases, no exact inference can be deduced from them; but I believe that my own post-mortem experiment on Owen Murphy is sufficient to make out a *primâ facie* case against the filter-theory, which ought to be tested by further experiments before it is admitted or rejected.

(*To be continued.*)

# PART II.

## REVIEWS AND BIBLIOGRAPHICAL NOTICES.

*De la Syphilisation. État Actuel et Statistique.* Par W. Boeck Christiania: H. J. Jensen, 1860. 8vo, pp. 72.

*Syphilisation.* Von Dr. Fronmüller, Aertzliches Intelligenz-Blatt. March 10, 1860.

*Syphilization.* By Dr. Fronmüller, Physician to Christ's Hospital at Fürth.

*Syphilisation.* Von Dr. Lindwurm, Aertzliches Intelligenz Blatt.

*Syphilization.* By Dr. Lindwurm, Physician to the Hospital at Munich.

The treatment of secondary syphilis by inoculation with the virus of a primary sore, or syphilization, as it is called, has attracted great attention on the Continent since Auzias-Turenne and Sperino published the results of their experiments some eight or nine years ago. Boeck, of Christiania, whose last pamphlet upon this subject we have been just reading with considerable interest, was one of the first to take it up; and so early as 1853, published a paper upon syphilization, followed by others, on the same subject, in 1854-56-57,—when we inserted in our February number for that year an original communication from Dr. Boeck himself,—in 1859, and finally, this last in 1860, in which, after giving a short account of the mode of using this new method of cure, the cases in which it is most likely to succeed, and those in which it ought not to be adopted, he proceeds to give a detailed account of 300 cases of secondary syphilis treated in the hospital of Christiania, by himself and his colleagues.

Dr. Boeck begins by asserting that syphilization, or inoculation with the secretion from a primary syphilitic sore, effectually and radically cures secondary syphilis—provided that the patient has not been previously treated with mercury or iodide of potassium, although this latter seems less antagonistic than the former to the treatment by syphilization. Of the 300 cases of which he gives a statistical account, only 10 suffered from relapse or were imperfectly cured; and *all* the patients so treated improved in their general health during the treatment. This important fact is also asserted by Dr. Fronmüller, of Fürth, who, though not so enthusiastic an upholder of syphilization as Dr. Boeck, yet, after giving this new system a fair trial in his hospital during the year 1859, when he subjected to that treatment forty-four syphilitic patients, declares that all the individuals thus treated, whether cured or not, were invariably improved in their general health and appearance. Dr. Fronmüller wisely confesses that "the nature of syphilization and its mode of action is not clearly understood, and its theory yet to be discovered." But what seems to be established beyond a doubt is, that secondary syphilis is curable by repeated inoculations with the infection taken from a primary sore; and that during the treatment, which sometimes is extended over many months, the most broken-down cachectic patients not only improve in health, but actually gain a robust appearance.

We cannot, therefore, be surprised if, on the strength of his successful treatment of 290 cases, Dr. Boeck exclaims—"This new method (syphilization) has at last realized the hopes of 350 years of experiments and researches for the discovery of a curative agent to replace mercury, which, at the same time that it expelled the disease, would not ruin the constitution of the patient, and be rarely liable to relapse." It seems to be pretty well ascertained that syphilization should only be practised upon such cases of secondary disease as have not been treated by mercury; and although iodide of potassium does not seem to have as great an antagonising effect as mercury, still those patients who have not been treated with either seem to do best under the new plan. Some German physicians, who have largely practised this treatment by inoculations, seem to think that tertiary symptoms are less amenable to it than secondary ones, and that with pregnant women syphilization is much less likely to succeed. Different views as to the mode of action of this repeated inoculation have been put forth in Germany. Many are of opinion that the inoculation of any matter whatsoever which could produce a pus-secreting

ulcer, would have as good an effect; but we feel with Dr. Fronmüller, of Furth, that we have yet to discover the theory of the peculiar action of syphilization, and that more experiments are required before we can, with any degree of certainty, lay down as law that this plan of treatment is, in reality, a super-saturating of the constitution with syphilitic poison, or only a clearing out of the morbid element by innumerable issues produced artificially all over the body. Two facts, however, have been pretty well ascertained, if we are to give credit to the numerous accounts published on the Continent—1st, that syphilization *cures secondary syphilis;* and 2nd, that it *is no prophylactic against re-infection,* as had been hoped by Auzias, Sperino, and many others, and is still believed by a few. Dr. Boeck, of Christiania, has given a list of 300 names of individuals on whom he practised syphilization, together with a description of those several cases and the results of his treatment, which proved completely successful in 290; and we shall extract from his pamphlet an account of his mode of proceeding.

He commences by cautioning physicians against employing syphilization in cases which have been previously treated by mercury; for in them the treatment by inoculation seldom runs an even course; and although eventually it will cure, yet he prefers that this treatment should be adopted solely in those cases which as yet have had no mercury. The same objection arises in cases previously treated with iodide of potassium, though in a less degree. The sooner the inoculations are practised after the appearance of the secondary symptoms, the more rapid will the cure be. The mode in which the inoculations are practised is as follows:—Having obtained some infection from a genuine syphilitic chancre, Dr. Boeck introduces it under the skin as in vaccination, selecting the sides of the thorax for the first inoculations, of which he makes three on each side. After three days he makes three fresh inoculations on each side of the thorax, *with pus taken from pustules resulting from the first inoculation*; and in three days more, he makes three fresh inoculations on each side of the thorax, with pus obtained from pustules resulting from the *last* inoculation; and so on he inoculates every three days in the same region, each time with pus obtained from the *last pustules,* until inoculation ceases to produce any result whatever. He now proceeds to inoculate over the two arms, continuing to do so as he had done over the sides of the thorax, until a point is reached where the poison introduced ceases to have any effect. He then passes to inoculate over the thighs, after the same man-

ner as had been practised over the thorax and arms; and, finally, he inoculates the patient over again with fresh infection obtained from other patients, until no effect whatever is obtained. In broken-down cachectic individuals, these inoculations have at times been followed by very ugly-looking ulcers, which have even in some instances taken on a phagedenic action. Dr. Boeck is evidently alive to this danger, and probably experienced it himself; because, though he does not mention it, he says, "When I possess some virus which has passed already through several individuals, I select it to commence with; but as the process of syphilization progresses, the more active infection may be employed without fear of its then producing phagedæna." Dr. Boeck places great stress on the locality to be selected for the commencement of these inoculations; and he asserts that the syphilitic virus has different degrees of intensity when applied to the different regions of the body, being most violent in its effects when applied to the thighs, and he therefore always leaves those for the last. Care must be taken, also, not to practise the inoculations too near to one another, lest they should become confluent, and by running into each other form ulcers of such size as to occasion risk of erysipelas and inflammation of the lymphatics, not to mention the risk of unsightly cicatrices. Although he generally practises three inoculations on each side every third day, yet he would be quite contented with one, if he were sure that it would take, as the safety of the patient does not depend upon the number of inoculations, but upon the simple fact that all inoculations of syphilitic poison cease to produce any effect. Three days are allowed to elapse between each inoculation, because less would be insufficient to produce pustules giving pus; and in some cases even more than three days' interval may be requisite. Often, with infants suffering from hereditary syphilis, the first inoculations will fail. In these cases the rule is to continue inoculating every second day until the virus produces a pustule, when it often happens that all the previous inoculations start into action. The same peculiarity is sometimes remarked with patients suffering from rupia, or some very extensive papulo-squamous eruptions. As to the local treatment, it is the same as in any other case of superficial ulcerations; and any mild dressing may be adopted, as circumstances may direct. With children it may be often necessary to administer some mild sedatives, to allay the irritation caused by so many little sores. In both young and old the diet should be abundant and nutritious, at the same time that alcoholic drinks should be avoided.

These are Dr. Boeck's rules and directions; and are followed by tables, wherein are detailed 300 cases treated by him in the hospital of Christiania. Are they to be depended on? We think they can to a certain point, if not in their totality; for we have them corroborated by no less an authority than Dr. Lindwurm, of Munich, who places special value on the writings of Boeck. He states, as we read in the "Aertzliches Intelligenz-Blatt," "Infants, adults, and old people have been treated by this method with success." The time required to effect a cure will vary according to the nature of the disease, its severity, its length of duration, the constitution of the patient, and the previous treatment. Lindwurm believes, from his experience, that from ten to twelve inoculations at a time, repeated every three or four days, will best lead to a cure. He does not, however, adopt this new method unreservedly; and he fully admits that it is an unpleasant, troublesome, disgusting, and by no means painless, mode of treatment; that it requires a long time; and finally, according to him, affords no certainty of cure. Its greatest value, in Lindwurm's eyes, consists in its always improving the patient's health, let that be ever so much broken down; "so that weakly, worn-out, patients become strong and robust under this process; and even weakly infants, affected with hereditary syphilis, become healthy and strong." But notwithstanding all this, Lindwurm grants to syphilization only a secondary place in the therapeutics of syphilis, still holding that in iodine and mercury we possess simpler and surer remedies, and which, with care and watching, will produce few disagreeable after consequences. But in such cases as broken-down cachectic constitutions, in which mercury would be inadmissible, Lindwurm would recommend a trial of syphilization. But by this process of inoculation is the patient really syphilized; is he, in other words, saturated with syphilitic poison, and thus rendered unsusceptible of taking in any more; or is he cured by a process of derivation brought about by the suppuration of some hundreds of small secreting ulcers? Boeck says, "In order to prove that syphilization is nothing else but a derivative treatment, its opponents return to the old doctrine of the duality of syphilitic virus. Simple chancre does not infect the constitution; and it is always with pus obtained from one of these that the inoculations are practised, simply because a hardened chancre (Hunterian chancre) does not, as they say, yield inoculating pus. The subject, therefore, who has been syphilized has only suffered from a vast number of simple ulcers, and the pretended syphilization is only a monstrous illusion—a complete error!

And are the hundreds who have been thus cured so many illusions also? Surely, if one had spoken thus ten years ago, and before experiments had been made on man, all those remarks might have been looked upon as very sage, and rightly deduced; but when now-a-days we hear long arguments against syphilization, based upon such grounds, one finds it difficult to listen to them seriously. Even supposing it is by derivation that the syphilitic symptoms disappear, is the fact of syphilization lessened thereby? The name—syphilization—might, perhaps, be badly selected, but the results would remain the same; and the idea of treatment by syphilization would have taught us that syphilis could be cured by derivation.

"But I may be told, perhaps, that there is no necessity in rejoicing over the discovery of the cure of syphilis by derivation, when we have in mercury a better remedy. Well, I can understand that syphilization be abandoned, if it is thought that mercury is both a sure and safe remedy—that would be rational; but if, on the other hand, it is admitted that mercury is not free from danger, I cannot comprehend how syphilization should be rejected because it is nothing else than *derivation.*"

In looking over the statistical tables which form the bulk of the pamphlet, we are struck by the high mortality of infants suffering from hereditary syphilis, who were subjected to this treatment. Out of sixteen thus treated, fourteen died shortly after its commencement, viz., one in two, one in nine, and the remainder in a period averaging between fourteen and thirty-five days. The average duration of the treatment of all the 300 cases was 144 days.

To us the most surprising fact connected with this new process, is the constant improvement of health during this continued inoculation with syphilitic poison. We say fact, because we find Dr. Boeck's statement to that effect nowhere contradicted; and, on the contrary, confirmed by Lindwurm, whose assertions in this matter we are compelled to receive with all the respect due to his well-established reputation. We cannot refrain quoting a passage from Dr. Boeck's essay, which appeared in the February number of this Journal for 1857: "It is a great physiological fact, that the syphilitic virus, by continued inoculation, annihilates itself; for this leads us to the result, that by syphilization we have entered upon a law of nature which can scarcely exist of the syphilitic poison alone. Other animal poisons probably must obey the same law." Dr. Boeck tells us, that "in some instances the

inoculated person becomes proof to one sort of virus; I then take the matter for inoculation from another, preferring a case which has had a different origin; this then proves effectual. But sometimes they become proof to this also, and then I seek for a third source; and thus I go on as long as any matter at all will operate." Dr. Boeck has a wholesome dread of mercury, without a corresponding faith in its curative powers. He says—"I do not think that any physician in Scandinavia would say, with M. Ricord, that in from four to six weeks he heals every constitutional syphilis." Now, we have to make great allowance for Dr. Boeck's feelings on the subject of mercury; for its action on the human economy is very different in high Northern latitudes than in more genial climates; and those who have only witnessed its action in these countries can form no idea of the irregular course it often pursues in Northern countries, and the serious after consequences it produces. Although syphilization seems so powerful in subduing secondary symptoms, it appears less efficacious in tertiaries; and Boeck states, that "in affections of the osseous system, syphilization hardly ever seems to produce any effect."

Dr. Fronmüller, physician to the Christlichen Hospital of Fürth, gives an interesting account of the treatment of syphilis by syphilization; and in the wards of his hospital, both men and women affected with chancres, condylomata, leucorrhœa, syphilitic eruptions, and buboes, were subjected to this treatment. He seems to have been highly successful, as the following cases, taken at random out of a long list of similar cases, will show:—"The first experiment I made was with a woman aged 32. She was admitted into hospital in April, 1859, suffering from ulcerations, with everted edges on the inside of her mouth and tongue; and also on the pudenda. She had considerable vaginal discharge. The neck of the uterus was, on examination, found to be small and pale; and the os was surrounded by numerous erosions, which bled at the slightest touch. Her neck and chest, and her back, even down to her buttocks, were covered with numerous copper-coloured patches. The patient seemed to be of good constitution. On the 2nd of May, I commenced the treatment by inoculating on the inside of each thigh with pus obtained from one of the chancres in the vulva. So soon as the following day, the spots which had been inoculated had become studded with pustules, each surrounded by a red areola. On the 4th of May, these pustules had acquired a diameter of a quarter of an inch; and six more inoculations were practised with the same infection as before, in the immediate neighbourhood of the first inocula-

tions. On the following day, the copper-coloured eruption was remarked to be paler, and the ulcers within the mouth were diminished in size. On the 6th of May, the pustules resulting from the inoculation of the 4th were as large as those of the 2nd. Six inoculations were made now on the thighs, with fresh matter obtained from the ulcers in the pudenda; and two inoculations were practised on the top of each arm, with pus from the ulcers of the mouth. By the 7th, the eruption had nearly faded away, whilst there was an abundant crop of pustules resulting from the inoculation of the 6th; and also some pustules appeared both on the chest and back, independent of any inoculation. On the 8th of May, six fresh inoculations were practised on the upper arm. By this time the ulcer on the left labium was healed. By the 10th, the pustules which had independently made their appearance over the chest and back were beginning to dry up; and the ulcer in the mouth had diminished in size, as also the erosions around the os uteri. On the 12th of May, the eruption was no longer visible. On that day two more inoculations were practised on the breast, with matter obtained from the ulcer of the mouth, which by the following day were surrounded with small pustules. On the 15th, fresh inoculations were made, but without success. The vulva was now nearly free from disease. The pustules of the first inoculations had dried up and fallen off, without leaving any scar. On the 2nd of June, the patient was discharged, cured."

"On the 12th of July, a patient aged 18, was admitted into Hospital suffering from œdema of the labia, chancres, vaginal discharge, and condylomata. After sixty inoculations, there remained only a slight discharge, and the condylomata;—after the 122d inoculation, the patient was discharged, cured."

"On the 16th of August, E. W., aged 29, was admitted into Hospital, suffering from redness of the pharynx, numerous ulcers of the labia, and condylomata round the anus. She was treated by syphilization, and from 4 to 8 inoculations were immediately practised on the inside of her thighs with matter taken from the ulcers. The inoculations were afterwards practised over the arms, and lastly over the sides of the chest, and below the breasts. Ulcerations as large as half an inch in diameter formed themselves on the inside of the thighs, and pustules soon made their appearance on the arms and sides, which by degrees acquired a diameter of half an inch, and in some spots became confluent. The progress of the treatment was most satisfactory: as the inoculations increased in number, so the ulcerations of the genitals first disappeared, and the condylomata diminished

in size; after the 80th inoculation, the ulcers were perfectly healed—all redness in the pharynx had disappeared, the vaginal discharge had ceased; and although the condylomata still remained, yet they were diminished to one half their original size. From this time to the 26th of September, about 70 inoculations more were practised, the last of which produced no effect. By the 30th of September, even the refractory condylomata disappeared. The patient was kept in Hospital till the 4th of October, in order to heal completely all the ulcers resulting from the latter inoculations, of which she had 150 in all, the 12 last of which failed to produce any result." This case relapsed, and it was the only one which did so in Dr. Fronmüller's practice. He says, "This patient, who had been dismissed on the 4th, returned for admission on the 16th. Ulcers had broken out over the spots where the inoculations had been practised, and the vaginal discharge, together with the condylomata, had also reappeared. Whether the relapse in this woman, who was five months pregnant, was due to a fresh infection, or whether it was the result of an imperfect cure, cannot with certainty be laid down." He does not state how this patient was eventually cured.

Dr. Fronmüller has evidently been made a convert to the new system, as can be clearly seen by his accounts of the many cases he has treated in the Hospital of Fürth, and from which we selected the foregoing ones. Stenberg also gives a confirming account, stating that by repeated inoculations of an individual with syphilitic pus, the chancrous sores which are produced gradually diminish in size, until at last, pus which shows itself fully capable of inoculating others, comes to have no effect upon the recipient.

Lindwurm, on the other hand, is only half converted to the new doctrine. He has succeeded in obtaining perfect cures, as will appear by the following case, which we give in his own words; but he has evidently some doubts in his mind, as will appear further:—"I admitted into my clinical wards, on the 23rd of last May (1859), a robust man, aged 35, suffering from extensive excoriation of the glans penis, with considerable induration of the urethral orifice, and numerous warty growths and condylomata, both on the penis and scrotum; he was also covered from head to foot with eruptions of various kinds, copper-coloured, papulous, and pustulous. Some of the pustules were dried up, and covered with a thick brown crust, especially about the nose, mouth, and chin. The fauces were free from disease. In addition to all this, the patient suffered also from chronic eczema, which he had for eight years.

Two hundred inoculations were practised upon this man, commencing first over the thorax, then over the arms, and lastly on the thighs, in which locality the ulcerations were deepest, and took longest to heal. Already, on the thirtieth inoculation, the patient was decidedly improving, and at the end of three months he was completely cured, without reaching the point of absolute immunity to all inoculation. He has remained in excellent health to this hour (March, 1860), and has even lost his old enemy the eczema."

Lindwurm was struck with the fact that his patient was completely cured of his syphilis without having become unsusceptible to syphilitic inoculation; and he determined upon repeating the experiments of Hjort, who, contrary to the opinion of Auzias-Turenne, Sperino, and others, who assume this treatment to depend upon a saturation of the body with syphilitic poison, believes it to be simply a derivative mode of cure, in which the syphilitic poison, *as such*, is quite unessential, and acts only as a simple irritant. Hjort produced a crop of pustules over the body by means of tartar-emetic pomatum, and obtained very favourable results. Cullerier and others, acting on the same principle, tried oft-repeated blisters of small size; every day from four to six small blisters were applied over different parts of the body, and by this means not only maculæ but papular eruptions, and even condylomata, quickly disappeared; squamous eruptions and indurations ef the glands, on the other hand, resisted this treatment. Lindwurm tried inunctions with tartar-emetic emetic ointment by itself in fourteen cases; in some, the results were most surprisingly favourable; in some, less so; and in some, no result whatever was obtained. The following cases, related by him to the Pathological Society of Munich, will be read with interest, both from the form of the disease, and the rapidity of the cure:—

"A childsmaid, aged 17, came, on the 24th of August, 1859, to seek admission into hospital for a pustulous eruption covering her forehead, neck, and arms; the glands of her neck were considerably enlarged, and she had a whitish and somewhat elevated ulcer on the inside of her upper lip. No appearance of disease could be detected in or about the pudenda, and her hymen was unruptured; in all probability, she had been infected by the mouth. The treatment consisted in daily rubbings with tartar-emetic ointment over the upper part of each thigh, until a copious pustular eruption was established. So soon as the first crop was dried up, a fresh inunction was made, until another crop of pustules appeared; and

so soon as these had dried up in their turn, a third inunction was practised. By the time this last eruption was dried up, all the syphilitic symptoms had greatly diminished; the glands were no longer swollen and hard, the throat was well, and only a few scarcely perceptible scars remained to point out the spots where the original pustules had existed. By the 8th of September even the ulcer in the mouth was healed, when she left the hospital perfectly cured.

"A housemaid, aged 19, suffering from superficial ulceration of the fauces, violent pains of one side of her head, as also from enlarged glands of the neck, with vaginal discharge, ulcerations of the os uteri, and condylomata, was admitted into hospital, and treated exactly as the previous case. Four separate inunctions were made on her groins, and also in three different parts of her thighs; and in seven weeks she was discharged, perfectly cured.

"The most surprising, however, of all the cures was one I saw in a man, aged 27, who was suffering severely from general syphilis. His breast, back, shoulders, and legs were thickly covered by numerous copper-coloured blotches, mixed with small indurated ulcers, while his genitals were covered with warty excrescences, and his anus surrounded by condylomata. He was treated exactly as the former case. After ten days a very decided improvement could already be seen; and in thirty-seven days, after employing four ounces of tartar-emetic ointment, he was dismissed, cured."

In cases of genuine indurated chancres (Hunterian chancres), as also in cases of psoriasis syphilitica, this treatment seemed to have no effect. Two females, affected with general syphilis, treated in the same manner with tartar-emetic ointment, were sent out of hospital, apparently cured; but in eight or ten weeks they came for readmission, the disease having returned in precisely the same form as that for which they had been treated. This time a modification of the previous treatment was adopted; and instead of inunctions with tartar-emetic ointment, issues were practised in the upper part of each thigh and arm, of the size of about a two-shilling piece, by means of Vienna paste (potassa cum calce). When the eschars had fallen off, there remained behind four deep pus-secreting ulcers, which were left to heal of themselves, and no other treatment was adopted beyond rest and good diet. They left the hospital perfectly cured.

"Now, if we compare the two modes of treatment, viz., that by syphilization, and that by derivation with tartar-emetic ointment, we find a similar result, with this difference only,

that the cure by tartar-emetic seems to be more rapid than that by syphilization."

Lindwurm, in conclusion, says that, apparently any derivative treatment, whatever the irritant adopted may be, will produce the same effect as by syphilization; but a much more extended series of experiments will be required, before any one shall be in a position to speak with certainty on this subject; and in the mean while, he considers that we possess in mercury and iodine surer and more appropriate remedies, which, according to him, ought not to be superseded by either syphilization or derivation.

Lindwurm is evidently half-converted by this new doctrine; but before he forsakes his old and endeared friends mercury and iodine, he wishes most properly to have still more evidence in favour of the new system Two facts, however, seem to be well established up to this, viz., that secondary syphilis can be cured by inoculations with the pus from a primary sore, and that patients so treated invariably grow strong and healthy under this process. Now, we cannot pass any judgment on this subject; we have no personal experience of it; but we have experience of mercury, and we cannot, assuredly, say as much for it.

We shall look forward with great interest for fresh information upon this subject; and should it eventually be proved that syphilis really anatogonizes syphilis, as Dr. Boeck is persuaded of, we should be inclined to believe, with him, that this new law in physiology would hardly be limited to one animal poison, but most likely be a fundamental law, ruling both over the syphilitic and other organic poisons also.

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*The Forms, Complications, Causes, Prevention, and Treatment of Consumption and Bronchitis, comprising also the Causes and Prevention of Scrofula.* By James Copland, M.D., F.R.S., F.R.C.P., &c., &c. London: Longman & Co. 1861. 8vo., pp. 440.

What are the "calamities of authors" to the calamities of those overworked beasts of burden, the reviewers? How it comes to pass that the literary camel gets through the work he does, and, without repining, carries what authors remorselessly put upon him, it would tax the most learned in zoology to explain. As for our camel, the faithful animal whose load is, at this moment, strapped upon our shoulders, can it be wondered at if his back has at length yielded to

the last straw? He never quite recovered the strain brought about by the last volume of the Cyclopædia of Anatomy. He was observed to despond as the Sydenham Society got upon him. The bantlings of Drs. Routh and Richardson he did not seem so much to mind; and, as a faithful camel, he stood up even under Mr. Cordy Jeaffreson. But now! Camels, it is true, were made to bear, but surely not such an incubus as lies before us. Such heaps of quartos, octavos, and duodecimos. Such piles of Pamphlets and Journals; of Essays and Remarks; of Observations, Suggestions, and Dissertations; pyramids of Replies and Disquisitions; of Elements; of Vestiges; of Transactions and Researches! The Human Skeleton from Cambridge. The Button Suture from New Orleans. Dr. Heale's Vital Causes, and the Constipated Bowels of Dr. Birch. Successful treatments of every infirmity that Adam the wicked bequeathed to his progeny. To wit—of Congestion of the Windpipe, from Hastings (with remarks on the sea-air); of something, and something else, without copaiba (for which may Mr. Weeden Cooke be made M.R.I.A.). Of Diabetes, by Dr. Camps. Is this author the eloquent and prolific speaker at the London Medical Society? Of Hemorrhoids by Nitric Acid, by Mr. Smith. Of Hemorrhoids without Nitric Acid, by Mr. Jones. Then images on all sorts of pedestals. Dr. Barker on the Tadpole-slippery? Dr. Beale, on the Urine;—where we are sorry to leave him. Dr. Combe, for the hundredth time, on the Management of Infancy;—a most difficult business, according to our experience. One inhuman medical Pickford, from Copenhagen, puts upon him "Statistike Undersögelser af laegeriders Kåbeligkt Inhold." A second foreign gentleman, "Den Kehlkoff Raehenspiegel;" and a third, "Islenzka Homöopathien og Nordlenzku Prestarnin. Ritad af Dr. Hjaltalin. Reykjavik!!" But the actual disgrace of breaking the camel's back was reserved, we regret to say, for an Englishman. Dr. Copland had found camels to carry his trunks. This one should bear his parcels; and—we put it to the Profession—is not this venerable camel-driver amenable to the Humane Society?

Not that we are unmindful that men of genius are privileged to boast a characteristic. And if that characteristic crops out to our damage, yet we will not forget that Dr. Copland is one of the lights—one of those few to whom it is given to stand on the platform in the sight of men. But then the Doctor's is such an awkward characteristic. Shakspeare had his many-sidedness; Keats, his sensuousness; David Hume was essentially emphatic; Alcibiades was adaptive—and Copland is

decidedly wordy. Now, though wordiness is a bore, yet we will remember the platform. Standing on that platform, we respect the *man*. If he have but glanced at one in a thousand of the authors he quotes in his Dictionary, he is an extraordinary man. If he remember but a small fraction of what he himself has written, he is a truly wonderful man. Old Birch, and Bayle the copious, are nothing to him. He must write swifter than Dumas, and know more languages than Mezzofanti. He is as insusceptible to fatigue as Hannibal or Lord George Bentinck. He is like—but with what can he be compared? He is like a bladder, full to bursting. Prick him on any one side, and out gushes a torrent of learning. Or he may be likened to the Mammoth Tree at the Crystal Palace, which you have to look at in sections, and wonder how it was made.

Yet it is our duty not to permit our judgment to be blinded by our admiration—however great this latter may be; and when, after we have staggered under four closely-printed volumes on every subject, known or unknown, that ever was or will be, could be, or should be, written, on medicine theoretic and practical; on pathology and the nature of disease; on the cure of disease; on morbid structure; on disorders peculiar to climates, to periods, and to sexes: not forgetting principles; a copious Bibliography; innumerable references, and a choice list of mottoes;—our author puts upon our back another parcel, having a title as long as a book need be—"The forms, complications, causes, prevention, and treatment of Consumption and Bronchitis; comprising also the causes and prevention of Scrofula!" is it to be wondered at if we feel that endurance has its limits, and that the time for remonstrance has at length arrived?

In our humble opinion, then, such a book was not wanted by any individual in the three kingdoms. It was not wanted, for it has no contents to recommend it; and, if it had, they have already been given to the world. *Un amour rechauffé ne vaut rien*; nor can a treatise on Consumption, founded on opinions and views of its pathology, diagnosis, and treatment held twenty years back, be subservient to any more useful purposes than the filling-up of a book-shelf, or enabling our eldest daughter to sit up to the piano. Such a book resembles a bit of amber in which is to be discerned an antediluvian fly. The contents are wonderful, because they are so very old. This work is of no use to the public, for it is too prosy. It is of no use to the physician; for, if he wants new views, he must seek elsewhere; and, if he wants old ones, he can get them in the museum. It is worse than useless to the student; for it gives

him false ideas of practical physic, and tends to lower in his estimation those principles, without which he cannot hope to become a successful practitioner.

Judging from the most salient feature of the work before us, it would appear, that in sad contrast to history and the exact sciences, medicine imbues with the most rampant egotism those who devote themselves to its pursuit. It would seem that, in our profession, it is impossible, as we get older, to avoid resorting in argument to that vice of depreciation—that supercilious contempt of youth and its efforts, which, in the absence of better weapons, patriarchs do so delight to wield. When Samson pulled down the pillars, we doubt not several aged Philistines went about, saying, "It's so like him;" and "what can you expect from a strong young man?" The strength and vigour of the disciples of Laennec and Louis must, we grant, be very distasteful to the old school of physic. It must be hard, we allow, to be instructed at the bedside by a young man *up* in cell therapeutics, the duality of a miasm, and the function of the posterior columns; but if the modern schools of pathology are to be upset, it will not be by the shoulder-shrugging and head-shaking, so much the fashion at the present day. The only parallel we can hit upon to the unseemly *animus* of the old against the young school, is in the wholesome example set by the heads of the sister profession towards those writers who give a new version of the Mosaic Cosmogony. If we had a Jowett, Dr. Copland would call him a Lapsarian; and if we had a Baden Powell, he would call him a heretic. Nor can the old school see anything unfair or unphilosophical in this. To them, the principles they imbibed young, are principles for all time. The mistress espoused in the heyday of youth is beautiful still to them; and it is impossible to teach them that she is partial to younger votaries. Novelty, as such, they detest; for they believe themselves to have been educated precisely at the right time, as far as the possible perfection of medicine is concerned. They are, consequently, sorry to say there is a rash way of dealing with disease. Successful treatment depends on a correct estimate of "condition of vascular action," and of the "circulating fluids," of which young physic is lamentably ignorant; and cannot be hoped for without a due reference to "the conditions of vital force, of vital energy, power, or endowment," which it is denied to any man under sixty to appreciate in the slightest degree. The young physician, forsooth, has his mind "preoccupied by the recognition of changes of structure and of palpable organic alterations," which shut out from his apprehen-

sion those principles which guided old physic to "practical results."

These tinkling phrases, "conditions of vascular action and of the circulating fluids;" "estimation of those conditions of vital force and of vascular action" recur from time to time, like the sound of the gong, with which, as they march to battle, the Chinese frighten barbarians. We should have thought the superior claims to medical prescience implied in these phrases had been sufficiently paraded in the preface; but, on dipping into the book, we hear the gong again at page 2.

"I can fully corroborate the inference drawn by my friend, Dr. M'Cormac, of Belfast, in his unpretending, but very able and learned, treatise on consumption, 'that the existence of tubercles may be inferred from the general aspect and condition of the patient,' before any physical examination is instituted—'before the patient has spoken a word, or emitted a sound.' The earlier, and *many of the more recent, cultivators of auscultation,* overlooked the fact that solitary or scattered tubercles in the lungs do not necessarily affect the sounds in the lungs—that tubercles do not always furnish a physical sign—in many even when the constitutional and physiological symptoms evinced the early stage of phthisis—the stage in which the recognition of the malady is most important, and in which a true estimate of these symptoms should be made and chiefly confided in: they relied *too confidently on the physical signs, and neglected the physiological and rational symptoms.*"

Is it not too bad that before we have recovered from this castigation, we should be doubled up again at p. 4? And our author, we think, in talking as follows, is somewhat rash in a commercial point of view. We are sure the old school think far too highly of themselves to dip into any works on medicine. The prospects, consequently, of a second edition depend very materially on Young Physic; but it is scarcely probable they will strive very hard to perpetuate so unflattering a portrait of them as is here drawn by the Doctor:—

"The early aberrations from health which indicate the commencement of tubercular phthisis, have been, during the greater part of the period of which of my experience is cognizant, either imperfectly estimated or overlooked; whilst attention has been, and still is, directed alone to that which, although fully deserving a due portion of attention, should not receive an undivided investigation —to *physical diagnosis* in the several forms and methods, in which it has recently been *paraded, over-estimated, and lauded.* Owing to this one-sided study, to the fallacies inseparable from its nature, and to those which arise from varying conditions of vital influence and action, from different states of secretion and excretion, from

numerous disturbing causes appearing contingently, and from *habits of dogmatising*, with the view of exhibiting a precision of acquirement and knowledge beyond what has been previously reached, the cultivation, if not the advancement of physical diagnosis, to the neglect of the intimate observation of constitutional and physiological changes, has been generally attempted. Manipulations which strike the senses of the attendants, and more than one sense of the patient,—examinations which may be seen, felt, and talked about, have a much more impressive and lasting influence upon both patient and spectators, than the close observation of symptoms, and the pertinent inquiries of the *profound and comprehensive thinker*. The former are lights which the possessor places upon an eminence for his own advantages; the latter are intended entirely to benefit the person for whose safety they are employed.

"Auscultation, which is of great service in the progress of phthisis, is much less advantageously employed at the commencement, and even during the early stage of the malady. Too great dependence upon, and a too *ad captandum* parade of, this mode of diagnosis, sometimes even with the fussiness and the flourishes of vulgar craft, have tended to the neglect of those states of vital manifestation, of disordered functions, and of vascular action, which, whilst they indicate incipient or early pulmonary disease, also characterise its forms, and point to the changes in which these forms originate, and on which they continue more or less to depend."

This is pretty language; perhaps a trifle too highly spiced. But how pleasant, is it not, to find such genuine philanthropy united to so much modesty! Sweet subject for contemplation —The *profound and comprehensive thinker* employing his lights, not for his "*own advantages*," but entirely "*to benefit the person for whose safety they are employed!*" For our own part, we feel guilty from head to foot; and thank our stars that so much forbearance towards us yet lingers, where the monopoly of diagnosis and power of treating disease are alone permitted to dwell. On the whole, however, and though we feel it to be a great honour that we should have our nose pulled by a Clutterbuckian, we would crave a little mercy in the next issue. Fancy being growled at through 450 pages. Adieu, microscope! Farewell, test-tube! Deceptive *ignes fatui*, see what you have brought us to! Had we but stuck to the old lights, what "practical results" we might by this time have been able to boast of!

We wonder if, in return for so much courtesy, we shall be thought wanting in all rules of *bienseance* and professional boredom, if we venture to probe the charges, and fathom the depths of the "profound and comprehensive thinker," who, if unlike

noble Festus, is not similarly affected by much learning, at least, in envy towards his younger relatives, successfully emulates the sisters of Cinderella.

The question, after all, lies in a nutshell. Does the pursuit of physical diagnosis, the application of the microscope to pathology and medicine, the rendering chemistry auxiliary to therapeutics, invalidate the pursuit of medicine as an art, render nugatory the cultivation of the senses, and, in fact, by transforming the physician into a mere reflector of physical impressions, disqualify him as a theorist for the discharge of clinical duties, and retard the progress of medicine as a science of observation? Is it true that physical and subjective medicine are opposed—that the successful treatment of disease can be based solely upon an apprehension of symptoms, which cannot be rightly interpreted by any one grounded as a chemist, proficient as a microscopist, or addicted to the physical method? Cannot a practitioner be at the same time "up" in the several kinds of fermentation, and able to diagnose enteric from typhus? Is it out of the question to suppose that a medical man can detect incipient cirrhosis, if he happen to be able to formulate the saccharine series? Does a knowledge of cytogenesis disqualify for discriminating carcinoma from fibrous tumour; or the ability to recognize a sarcina, make it impossible to distinguish apoplexy from meningitis?

This is not unfairly stated. Either the study of symptoms is everything in medical education, the records of mere observation and experience inefficient for clinical purposes; or far too lavishly have the favours of the public been showered upon the Graveses, the Stokeses, the Brights, the Bowmans, the Walshes, the Williamses, the Johnsons, and the Todds. These are they who personify the triumph of the inductive school. They are evidences, not so much of individual success, as of the invincibility of the principles which guide them, of the principles they teach, of the principles enlightened minds unhesitatingly acknowledge—of those principles, in short, which have lifted medicine, hesitating, dogmatic, blundering, and opinionative, to the rank of an exact science. If it is to them that, in pathology, we are indebted for our knowledge of the nature of tumours, the occurrence of fatty degeneration, the conversion to amyloid, the regeneration of bone, the cycle of inflammation, the results of elimination on secreting texture, the process of albuminoid softening;—if, in physiology, for what we know of the functions of nerve centres, the nutrition of the ultimate cell, the generation of animal heat, the formation of sugar, the production of pigment,

the secretion of the pancreas, the influence of the sympathetic upon temperature, the periodic evolution of the human germ, and the true nature of epilepsy—if, in clinical medicine, for the differential diagnosis of cardiac and renal dropsies, of true and false chancre, of cerebral decay and extravasation, of diseases of retina and choroid, and of gouty and simple inflammation—shall we say that their work is valueless, and that they are successful under false pretences?

If Dr. Copland can abstract himself but for one moment from symptomatology, the sole object of his devotion, he will be led, we are confident, to acknowledge that beauties are to be found elsewhere. Dr. Copland is perfectly aware, for example, if he will refer to his own Dictionary, that the urine frequently deposits sediments having general characters almost identical. Should he, however, attempt to discriminate between them by the aid of his unassisted senses, however acute they may be by long cultivation, he will in the majority of instances find himself considerably at fault. He might touch them, smell them, and taste them, and the sole result would be that he would be unable to eat his dinner. How would he decide between albumen and earthy phosphates; between urates loaded with purpurine, and the oxalate of lime? Could he detect sugar in urine of low specific gravity without the test-tube? Could he diagnose a wasting, or a fatty kidney without attendant dropsy, if he dispensed with the microscope? or decide without this instrument that coagulation was due to the albumen of pus or blood if present in small quantity?

Now, what the microscope and the test-tube do for diseases of the balancing organs, the stethoscope and physical diagnosis do for diseases of the engines of life. We cannot understand how a physician in a right frame of mind can bring himself to disparage and treat derisively the means placed at his disposal by workers of the last thirty years for the investigation of chest disease; nor do we envy him the pangs his conscience must inflict for his systematic rejection of them in practice. We should like to meet the Doctor over a case of aortic regurgitation, or aneurism masked by chronic valvular imperfection. Would a diagnosis be arrived at by a due regard to the "vital force, power, or endowment," any more than to the colour of the hair? How will he decide between dilatation and a mitral regurgitation? How conclude between the former and a chronic pericardial effusion? between an old and a recent endocardial murmur? or, as simple a case as can present itself, dispense with fremitus, respiratory charac-

ter, and percussion, in diagnosing between pleurisy and consolidation?

And a strange contrast at once presents itself, in cruel mockery of the venerable views of Dr. Copland, if we consider the power in tracking disease, that we possess on the one hand, over the heart through its movements, the lung through its murmurs, and the kidney through its tell-tale stream; and, on the other hand, over those thoroughly respectable organs, the liver, spleen, and pancreas, which so discreetly keep silence when physicians libel them. Take, for example, the liver. Here is an organ resisting all attempt at physical investigation. As inaccessible as How-qua, it might, for all purposes of exact medicine and clinical progress, be like that celestial flower, in the moon. Like the King of Siam, it not only says nothing to anybody, but forbids anybody to say anything to it. Occasionally it protrudes a margin, and there's an end of it. It has, we are told, a secretion; of which, however, it declines to afford a sample. Accordingly, so little do we know of its pathology, that we scarce are informed of its function. In deciding, consequently, on its diseases, we are driven to symptoms, and symptoms solely. And what do they do for us? What do we purpose, when giving cholagogues? A pain in the shoulder?—Liver! Headache?—Liver! Black spots before the eyes?—Liver! "My dear Madam," says the Patriarch, "you are bilious—bilious, Madam, not a doubt of it." And though, of course, what such an authority says must be true, yet we would venture to ask each individual member of the profession, singly, and the entire medical body politic and corporate, collectively, what the word *bilious* means, and why, if it infers "flow of bile," we give mercury, which is, or was, supposed to aggravate such flow; and why, if it is not "flow of bile," we give precisely the same remedy.

Similar observations apply, when we reflect upon our knowledge of morbid changes in structure and function, of the pancreas and the spleen. Look through volumes of medical journals, go through ward after ward of hospitals, and you neither find narrated, nor see a single case diagnosed, "pancreatic or splenoic disease." And why? Not, surely, that the pancreas or spleen enjoys immunity from disease; but that to the function, or the perversion of it, in either organ, we cannot bring the test of physical diagnosis. The brain is not so bad. It will often give tokens of the disturbances occurring within it. Yet oftener still does a perversion of its functions inflict nameless tortures, conjure up horrible terrors, or curse its owner with loathsome hypochondria, while the physician

can do nothing—powerless without the physical method. How many of us can detect and map out a lesion of the medulla above the anterior pyramids? Who can localize the *fons et origo* of a facial palsy, of choreic irritation, of epilepsy? We experiment on everything with life. We turn ourselves into Manchoos towards legions of rabbits and frogs, but do not arrive at a tithe of the knowledge of these organs that we have of the others; for comparative and experimental physiology will never tell us of them, what a drop of potash to the urine tells us of the kidney, or listening one moment with the stethoscope tells us of the lungs or heart.

The great truth at which those whom Mr. Meredith would call the WISE YOUTHS have arrived at is, to express it as a wise youth should, aphorismatically—*All disease involves anatomical change.* The question then presents itself, how to detect such change. Clearly by noting altered properties of substance and texture; alterations in elasticity, in density, in conducting power, in dimension, in physical and chemical characters. Such alterations always precede symptoms. A cardiac murmur is heard before dropsy displays itself. Sugar is present in the urine long before thirst and ravenous appetite arrest the attention. Anatomical change may or may not be susceptible of accurate discrimination; but its detection, at all events, is accomplished at a much earlier period, and with far greater accuracy, by observing altered physical qualities, than by the recognition of "vital functional derangement."

The recognition of such changes is often very difficult, and thus far we are bound to sympathize with Dr. Copland. It is essential there be a skilled sense. But there must be more than this; for the signs in themselves are of no value, however accurately the sense may perceive them. The physician must be able to couple these signs with the condition originating them; and then, by means of a higher calculus, deduce *their* pathological bearings and character. To tread so far in the path of induction is no mean thing. For to know every step of the road is to solve and compass the problem, DISEASE; and were the *method* applicable to the remedies, man would be its master.

If then it be true, that in a clinical point of view the objective are of far higher value than the subjective phenomena of disease, it naturally follows that this superiority will be most marked in diseases of the chest; and this we believe is conceded by most physicians of the present day; for the lungs manifest physical signs during the whole of life, and under every condition of rest and motion, of health and disease, that

can possibly occur. Yet, in the book before us, the physical signs are, if not abjured, certainly disparaged; and the detection of the several stages of phthisis made to depend upon the symptoms! and this in the latest work of an authority—the work of Dr. Copland—of Dr. Copland, no longer the compiler, the accumulator of other men's wisdom, but the *teacher!* To account for such a fact as this, surely we shall find some startling symptoms of which the profession has hitherto been ignorant—symptoms which enable the author to dispense with signs in discriminating between capillary bronchitis and lobular pneumonia; between the latter without fremitus, and pleurisy; between the latter in the dry stage from pleurodynia, with bronchial irritation.

So that none of them may escape, we append them all. The treasures of experience are, in the book, respectively labelled A, B, C, &c., like the priceless porcelain goblins in the Dresden Gallery.

Treasure A.—*Quickness of respiration.* This is, as described by Dr. Copland, a sort of chameleon symptom:—"In the more latent and chronic states of the disease, it is not experienced unless more or less physical exertion be used;" and yet we have it, that "as the malady advances, respiration is very quick, the acceleration being much greater in proportion to that of the pulse than in health, or at an early stage."

Treasure B.—*Observations by the spirometer*, kindly furnished, not by the author, but by the Hospital for Consumption.

Treasure C.—*Hæmoptysis*, "a more or less dangerous occurrence," p. 29, "not often a very dangerous symptom," p. 40. It may occur "unexpectedly," or "suddenly," delicate alternative; and "immediately upon, or soon after, more or less physical exertion." We should have thought the above included every possible circumstance under which hæmoptysis could occur. But no! It may "on some other occasion" not be "preceded by any marked indication."

"When, however, circumstances admit of the occurrence, or of the appearance of premonitory symptoms, they" (the circumstances?) "are chiefly chills or *horripilations*, of the general surface, sometimes followed by redness and heat of the face, *or flushings of the cheeks*"—i.e. the face without the nose—"a feeling of internal warmth, particularly in the chest; pain or tension at the epigastrium or hypochondria; a burning sensation under the sternum, with more or less anxiety, inquietude, constriction, or oppression at the chest, or dyspnœa; a short dry cough, dyspnœa, or

shortness of breath on slight exertion; a dull pain or soreness under the sternum" (poor party, he must suffer terribly about the sternum), "between the shoulders, or beneath the clavicles; palpitations; a quick, hurried, or excited pulse, which is sometimes also hard, full, bounding, or oppressed; headache; coldness of the extremities, with a collapsed or empty state of the veins of the surface; lassitude, and sense of weight of the limbs; occasionally cramps, or spasms of the lower extremities; flatulence, or borborygmi; costiveness, or pale urine."

Of a surety, Dr. Copland, when he visited one mortal with all these torments, must have been getting up Dante's Inferno.

Treasure D.—*Cough*, which is to be considered phthisical according to "the state of breathing and the appearance of the features, *particularly of the eyes*." Further, we learn, that "gastric cough is readily mistaken for the *early stage* of phthisis"! and that "nervous cough is often occasioned by masturbation."

Treasure E.—*Expectoration*, whose characters pathognomonic of phthisis Dr. Copland states "are the striated mass, with a mixture of whitish fragments in it, and the ash-coloured globular masses which are observed in the more advanced stage." The quantity may be "almost altogether absent;" and "the cases of this description which occurred in my practice had been mistaken, *before I saw them*, for low nervous or typhoid fevers." With respect to calcareous concretions from the lungs, we are spared any of those dry details that could only serve to throw light on points of retrograde phthisis, peculiarly interesting at the present day, and are favoured instead with a touching reminiscence of a medical friend who spat up such things, and who was sent up the Mediterranean, in the first stage of phthisis, for change of climate, in the year 1818. He is now, we rejoice to say, alive; and we venture to congratulate Dr. Copland on his having, for so many years, enjoyed the friendship of such a gentleman.

Treasure F—is *Pain*, which, "when the bowels are disordered, may be caused by the state of the colon."

Treasure H.—*Pulse*, there being no treasure G.; it having, doubtless, been stolen for its value.

Treasure I.—*Hectic fever.*

Treasure K.—*The digestive functions;* in which all is said about such matters.

Treasure L—affords an opportunity for a brilliant solo on the gong. Emaciation justifies the inference of Phthisis, "when it occurs without manifest cause, under which circumstances

the *fuss*, *parade*, *manipulation*, *and charlatanry* of a physical examination of the bare chest, so often unnecessarily and even injuriously practised, may in most cases be dispensed with."

Treasure M.—*Clubbing of the fingers.* The doctor affirms this to be one of the most valuable gems of the collection. We think it paste. We know many persons with clubbed fingers who threaten to live as long as our friend, who "went up the Mediterranean in 1818."

Treasure N.—*Œdema of the extremities*, which, when amounting to anasarca, renders it *not unlikely* (*!*) that there may be "albuminous changes in the urine."

Treasure O.—*Morning perspirations.* "When they appear, the second, if not the third, stage of the malady may be considered present. When this symptom is manifest, there can be no question of its nature."

Treasure P.—*Aphthæ.*

Treasure Q. This is the last treasure, and refers to the hair, with which naughty ones part very speedily, it seems; for "it often falls out very early, when the malady has been caused by masturbation, or premature or excessive sexual excitement."

And these are the symptoms which are to have priority of physical signs in the recognition of the several stages of Phthisis! We have never seen a smaller mouse produced from so big a mountain. The only symptom here mentioned, as discriminating the first and second stages, is morning perspirations; and is there any mortal man that ever felt a pulse, that would abide by such a symptom? Is it not universally conceded, that there is no symptom pathognomonic of a stage? We have seen, not one, but scores, of phthisical patients with complete dryness of skin. As for the "quickness of respiration," will anyone volunteer a corroboration of Dr. Copland's view, that the respiration becomes quick as the disease advances? We should, on the contrary, say that the disease manifests no such symptom; and it is certainly altogether at variance with fact, that the acceleration is greater than that of the pulse. Had Dr. Copland made a shot at pneumonia in speaking of the pulse-respiration ratio, he would have been nearer the mark.

We have scarcely patience to observe, that in our opinion nothing can be more trivial than the endeavour to give a clinical importance to hemoptysis as a means of detecting phthisis. Of course—for no one now believes in phthisis ab hemoptoe—when observed, it can only be sequential to rupture of tissue, to the attainment of such a phase of the disease as excludes the

hope of retrogression. And even as a symptom, the description of it is most imperfect and inaccurate. It is hematemesis that is attended with pain at the epigastrium, *not* hemoptysis; and something more than the state of the pulse, the paleness of the urine, and the inconvenience from borborygmi, are necessary to arrive at the conclusion, that blood is not being poured out by the stomach. We require more information than Dr. Copland vouchsafes, to decide on tubercles from the presence of mere bleeding, since it is possible for the blood to be florid from a large artery in the stomach, and for the blood to be frothless when voided in large quantity from the lungs. One of the most common corroborating symptoms, which is not mentioned, is the salt taste in the mouth. There is no mention of the influence of pregnancy upon phthisis; the habitual frequency of the pulse; the red line on the gums, insisted on by Dr. Thompson; periodicity in alteration of weight; and the relation of phthisis to glycoemia.

All these symptoms, however, are but the results of constitutional irritation; and it is easy to see that a large amount of deposit may be as productive of constitutional disturbance as a small amount of deposit in a softening state. Symptoms are merely the result of general disturbance secondarily induced; physical signs speak directly from the affected organ, and lead at once to diagnosis, to prognosis, and to treatment.

In nearly every possible case, the physical signs of the first stage may be held to be—in females, percussion dulness, with deficient expansion movement; in males, intensified vocal fremitus. Any one of these signs, if strongly marked on either side in either sex, is, excluding pleuritic adhesions in the case of movement, if not pathognomonic, more than sufficient to afford a clue to the interpretation of symptoms. If clearly established on the left side in the male, intensified fremitus is a most valuable physical sign. Marked dulness shows a deposit, which, of course, as the right side gives out naturally a deader sound than the left, is most valuable if established on the left side, and is always present and to be detected, barring emphysema. In males, then, intensified fremitus, in females, deficient expansion, when allied with weakness of respiratory murmur as compared with healthy side, little clearness, interrupted and bronchial breathing, indicate the first stage of deposition and induration. What vocal vibration and deficient expansion with exaggerated elevation movement, do for the first stage, rhonchi and marked dulness do for the second. If the rhonchus partakes in the least of a cavernous quality, and the respiration is at all blowing, the diagnosis cannot be uncertain.

In the third stage, though a stratum of healthy or pneumonic lung may disguise the cavity where slight percussion is used, absolute dulness is to be elicited on strong percussion. If the vomica be empty, blowing or cavernous breathing may be heard; if not empty, the pectoriloquous character of the voice, gargouillement, and exaggeration of signs of first and second stages, exclude all chances of error.

We have not, in what we have here observed, attempted to affirm the infallibility of physical diagnosis. We question much whether medicine would materially benefit by the perfection of the physical method, *pure et simple*. There would be no room for a difference of opinion, so delightful to the old school. There would be no scope for those amiable *banalities* about power, vital endowment, and the constitution. Luckily for the peace of mind and self-gratulation of the oldest of the Achaians, physical diagnosis is in its infancy. There are but few even of its professed advocates who have educated the sense to the detection of the finer murmurs. And among the adepts, we are free to confess, it is possible for a diagnosis to remain uncertain, and for physical signs occasionally to mislead. Thus it is difficult, sometimes, to arrive at a diagnosis where bronchitis ushers in the tubercular disease. Phthisis is often masked by the co-existence of emphysema. We all know that the cracked-pot sound is not to be depended upon in all cases of cavity; that it may be present even when the lungs are healthy, and is not uncommon in pneumonia, with and without effusion. But the physical method will never allow us to ascribe, as we have seen done, the dyspnœa and weakness of amenorrhœa and spinal irritation to phthisis; or the fever and crepitus of the last stage to an attack of acute pneumonia. And in all doubtful cases, the practitioner skilled in the physical method enjoys an unmeasurable advantage over the practitioner judging merely by symptoms. For the former has these with which to check his results; the latter abides by the impressions these symptoms convey—symptoms which too often manifest themselves but at the last and irreparable stage of the disease, which frequently are common to many diseases, and which, in all cases, afford boundless scope for every possible fallacy.

It is not to be wondered at that Dr. Copland should give us no information on the histological character of the sputum. No man is wise at all hours, or on all subjects, or even on all branches of one subject; or he would have shown us wherein he differs, for he is sure to differ, from Dr. Andrew Clark and Professor van der Kolk. It cannot be of any con-

sequence to Dr. Copland what this latter gentleman thinks on the subject; but we have heard it said, that the Professor insists much on the diagnostic value of the expectoration; and that by a microscopic examination of it a "practical result" is arrived at, of a not unimportant character. He believes that even when physical signs are altogether absent, the elastic fibres of the lung are to be detected at the very commencement of the disease, and at the first formation of a vomica. It is, no doubt, very rash of van der Kolk to go so far as to affirm this sign to be one of the most valuable in incipient phthisis; and we have no doubt, that at Dr. Copland's suggestion, both he and Dr. Clark will recant so flagrant a heresy.

It was with eagerness we turned to the seventh chapter, wherein, as we observe in the table of contents, are to be found the "author's own observations on the state of the blood in phthisis." We entertained the rash idea that the chemical theory of tubercle would be sifted; that the non-absorption of fatty matters would be explained, and those subtle phenomena of albuminoid selection be rendered easy things to understand. The vexed question of the inflammatory or non-inflammatory nature of the deposit; its distinctive feature as compared with other adventitious products; the reason why it has no inherent powers of growth; the presence or absence of nucleated cells in tuberculous matter; his own private opinion on the inter-corpuscular substance; whether there be a true blastema; why tubercles take so kindly to pigment as they grow older; and whether they can be said in this condition to approximate to cancer. Information on all these points will, no doubt, be detected by the earnest inquirer in the appended exposition of the author's own views:—

"I may adduce the following as the results of my own observations of the state of the blood in the early and in the advanced stages of pthisis:—At an early period, or even before the disease has fully declared itself, the blood is thinner or poorer than in health; the colourless globules are more or less abundant, and the red globules less numerous; the clot is somewhat smaller, its crasis less, and it sooner loses its cohesion. As the disease advances, and as febrile action is established, the fibrin is somewhat increased, and this is more certainly the case, if hemoptysis, or intercurrent inflammations of any of the pulmonary structures take place; the red globules are diminished, and the albumen and fatty matter are not very materially changed in quantity. The alkaline salts are slightly deficient, and lime is somewhat in excess. It should not, however, be overlooked, that, among the numerous analyses of the blood in phthisis, there are very great differences in the quantity of fibrin,

of albumen, and of fatty matter. I have here given what appears to be the more correct results. Probably the quantity of each of these is not so different as the quality, the intimate constitution, and vital relations. In the last stage of the disease, the blood appears still more watery, owing chiefly to the deficiency of red globules: and the colourless globules more numerous than in health. The greater abundance of colourless globules is probably owing to impaired assimilation, or metamorphosis, of these into red globules. The colourless globules in this, and indeed in earlier stages of the disease, have been mistaken for pus globules, the existence of which in the circulation is doubtful, or at least not satisfactorily demonstrated. The vital crasis, as well as the size of the clot, progressively diminishes, and the coagulating power or force of the fibrine is much diminished. It does not appear that the *microscopic appearances* of the blood in phthisis are different from those now stated, *or that the observations which have been made with the aid of this instrument have furnished any additional facts to those now adduced.*"

"That the *vital crasis* of the blood is *specially altered* in tuberculosis and consequently in pulmonary consumption may be admitted; but in what the true nature of this alteration consists is beyond our present knowledge. We can only remark the appearances it displays, and the states of *vital force* and of assimilation and nutrition with which it is connected. That the tubercular deposits in the lungs or elsewhere, which certainly present the feeblest traces of organisation, even in an early stage of their formation, if indeed they possess any whatever (for both opinions have been espoused), should consist of a modification of fibrine, as supposed by Rokitansky, requires further confirmation. He ascribes the formation of tubercles to an arterial elaboration of the fibrine, and contends that, in consequence of the alteration of the fibrine, tubercle is continually deposited, even when the blood is deficient in that constituent, the fibrine that is formed being soon affected by the peculiar dyscrasia and deposited in the form of tubercle. The rapid coagulation of the tubercle-blastema, or the fluid from which the tubercle is formed,—for it must be effused in a fluid form,—its tendency, after coagulation and growth or accumulation, to soften, he believes to be favoured by an active arterialisation, and prevented by a venous condition of the blood—changes, which, in his opinion, indicate an affinity between tubercle and fibrine. This hypothesis derives its chief support from the circumstance that debility is found to increase the quantity of fibrine, and also to favour tubercular formations; but it carries us no further in our knowledge than was previously admitted—than that the increased quantity of fibrine in the blood of scrofulous and phthisical persons, arising either from the imperfect assimilation, or from the waste of the blood-globules, or from the waste of the tissues, being imperfectly endowed by the vital force, owing to its low grade, is insufficiently elaborated and eliminated, and *is deposited in the form of tubercle* in situations favourable to its accumulation, where its imperfect vital

endowment and elaboration favour the changes it subsequently manifests."

These are the author's *own* observations; and we beg they may be received with the deference due to "practical results." We should have imagined, had not Dr. Copland expressly told us otherwise, that these, for the most part, were Rokitansky's observations, and not Copland's. They are, however, the author's *own*; and, as they are his own, we suppose, like the Duke of Brunswick's Own, they want no quarter. But what the Doctor declines we will in deference bestow. Otherwise we might confess we do not see much in them, and might venture a hint that tubercle is as much a cancerous exudation as a fibrinous, inasmuch as both these products have nuclei, and a tubercle corpuscle has not; that tubercle exhibits no resemblance to advanced fibrinous structure, either plastic or purulent, inasmuch as it presents no trace of organisation on the one hand, nor does the size of the tubercular corpuscle or its affection by acetic acid correspond in any way to the pus globule on the other. As a rule scarcely without exception, and then only when found among epithelial structures, tubercular matter presents no nuclei. This absence of nuclei, and presence of granular matter and molecules, holds equally for firm and soft tubercle; and the same elements are found to constitute the grey granulations, differing, notwithstanding, as these do so remarkably in appearance.

Nearly one-half of that portion of the work devoted to phthisis is given to treatment and remedies. It is edifying, if of doubtful practical value, to learn how phthisis was treated 100 B. C., and to find some countenance for our present differences of opinion in the want of unanimity in physicians of all times. Thus, those well-known practitioners, Oribasius, Actuarius, and Nomrus, advocate blood-letting; while Octavius, Horatianus, Cælius Aurelianus, Marcellus, Dioscorides, and Paulus, lend their authority to the administration of such elegant preparations as frankincense, pomegranate, and blood-stones. Celsus, we find, advises the cautery on various parts of the chest; and the ulcers *not to be healed as long as the cough continues.* Stolid old Roman! This is something like a "practical result." The elder Pliny was partial to goat's fat in gruel, evidently the cod-liver oil of the day. Galen invented a lozenge of *Scribonius Largus,* a title whose mystery gave it, no doubt, claims on public favour almost as strong as those of the Revalenta Arabica. (It appears, further on, that Scribonius Largus was an individual, not one of the materia medica.) Cælius,

like many foreigners of our acquaintance, considers the cold bath dangerous, and advises sailing to a distant climate. Otius was in soul evidently an alderman, for he had the highest opinion of venison fat dissolved in soup; and Schenck, who doubtless had attained higher civic dignities than that illustrious predecessor, is strong in praise of turtle broth, and snails fattened on sugar. Dr. Copland avails himself, in his pœan on Bennet—not Hughes Bennett—of the opportunity of rebuking us once again. Bennet recommends Welsh flannel next the skin, "not to be too frequently changed;" and, says the Doctor, "If we except the recent employment of cod-liver oil in phthisis, in what, it may be asked, has the treatment of this disease been advanced, since the appearance of the work of Bennet, by the *voluminous writings of specialists and stethoscopists of recent times?*" Dover, we are told, advises a frequent bleeding, horse exercise, crude quicksilver in large quantities, aniseed and crocus martis made into pills, with the balsam of locatelli in the morning, and elixir of vitriol in the afternoon.

Dr. Copland's own treatment consists, if hæmoptysis is present, in sponging and the exhibition of turpentine. Cough he relieves by conium and camphor, and the *anisate of sulphur.* "In preventing, counteracting, or removing the morbid conditions of the circulating fluids in the advanced stages of phthisis," the boracic and dilute phosphoric *acids.* The *alkalies*, "more particularly, when the blood is probably more or less contaminated by the passage into it of morbid matters from the lungs." Ammoniacum and other gum resins, the fish oils, Carrageen moss, and issues, have his approval; and bleedings, under certain conditions, are not discountenanced. We append Dr. Copland's form for administration of turpentine:—

"℞.—Olei Terebinthinæ ʒij. ad ℥ss.; Camphoræ gr. vj. ad gr. xii.; Spirit. Ætheris Sulph. Comp. ℥ss.; Tinct. Lavand. Comp. ʒiij. (vel Tinct. Guaiaci Comp. ʒiij. vel Tinct. Cinchonæ Comp. ℥ss.); Olei Cajuputi *m*xv. ad *m*xx.; Tere cum Mucilag. Acaciæ ʒj.; Pulv. Tragacanth Comp. ʒij.; Pulv. Glycyrrh. ʒss.; Syrupi Rosæ, et Syrupi Tolutani, āā, ʒj. ad ʒij.; Aquæ Rosæ ℥ij.; Aquæ Destillatæ (vel Infusi, vel Decocti Cinchonæ, vel Infusi Cascarillæ) ad ℥viij. Misce. Fiat Mistura hujus sumatur Cochl. j. largum, omni horâ, vel 3tis, 5tis, vel 6tis horis, prius agitatâ phialâ.

Dr. Copland insists strongly on the benefits to be derived from change of air, "travelling and voyaging." As to the last of these, voyaging,—

"Very well founded expectations of success from it may be

formed, if it be commenced in this stage, or before the second be far advanced, more especially in cases which have been attended by hemoptysis at their commencement or early course, and if it be continued for a sufficiently long period."

A decent income, we presume, being taken for granted,—

"Voyaging in the Mediterranean or in the Atlantic between the degrees of latitude, 30° to 50°, and preferably in the Pacific Ocean, especially when prolonged, either in naval cruisers or by repeated voyages, so as to avoid the winter and spring of this and other countries unfavourable to consumptive patients, deserves to be more frequently recommended than it has hitherto been. Now that the passage across the isthmus of Panama is easy, voyages thence, in various directions in the Pacific Ocean, may be made, and a return to this country effected in May or June."

We conclude by quoting the Doctor on a most important article of dietetics, which would have made the elder Disraeli's mouth water:—

"*Milk* has always received great commendation in phthisis, but writers have differed respecting that which is most beneficial. Thus Zacutus Lusitanus and Blegny prefer *human milk;* Aretæus, Burserius and Stoll, asses' milk; Diemerbroeck and Velschius, goats'milk; Schenck, either asses' or goats' milk; Hippocrates, either *mare's* or asses' milk; and Heister, the whey of cows' milk. Stoll considered that *asses' and human milk should be diluted,* and that they are injurious in the inflammatory complications and in the last stage of the malady. Whatever may be the diet and regimen adopted, milk of various kinds, in suitable forms and states of dilution, constitutes an important part of the treatment of phthisis."

In bringing these remarks to a close, we hope Dr. Copland will acquit us of any desire to inflict pain, or to treat inconsiderately labours to which he has devoted so many years, and which he has pursued with such unremitting energy. As a man of vast learning, Dr. Copland enjoys the respect of every member of the profession; but no amount of learning can compensate such an utter want of judgment—a deficiency Dr. Copland never more needlessly or in worse taste displayed, than when he bethought him to characterize as charlatans those professional brethren who put a higher value than himself on the diagnosis of disease by means of physical signs.

*On the Origin of Species by means of Organic Affinity.* By H. Freke, A. B., M. D., &c. Dublin: Fannin and Co. 1860. 8vo, pp. 135.

When Dr. Freke's publication was placed in our hands for review, we were struck with the similarity that its title bore to Mr. Darwin's most interesting book. We had, however, not gone far, when we discovered that we were dealing with a very different writer, and that the similarity ceased the moment we passed the boundaries of the title-page. Indeed, no two books could be more dissimilar; and while Mr. Darwin's might easily find a place even on the elegant shelves of a lady's boudoir, Dr. Freke's was only adapted for the library of the philosopher and the student.

But why have called this little book "The Origin of Species," while it treats of no such thing? Mr. Darwin's work, indeed, pretends to account for how the present races of animals and plants have established themselves; but Dr. Freke does no such thing, and only attempts to show how life may have originated on this planet, when, during that period of solemn misty twilight, before the great luminary that rules the day had yet pierced through the heavy canopy of vapours that still enshrouded the earth—"The Spirit of God moved over the face of the waters."

We regret that Dr. Freke should have adopted so obscure a style of diction, as it only serves to render still more difficult a subject already difficult enough to understand.

Several years ago we reviewed a work of Dr. Freke's on "Organization;" and we were pleased to find that, in an age when men are apt to manufacture books by the aid of scissors and paste, he had started new and fresh ideas of his own, and, leaving the old and beaten track, had thought for himself. In the present work, which contains much of what might not inaptly be called the metaphysics of physiology, Dr. Freke has followed his old plan of thinking for himself; and though we cannot follow him everywhere, still we give him credit for his ingenuity, and the boldness he shows in expressing it, of which the chart placed at the commencement of the work is, perhaps, one of the most striking proofs. At first it was to us totally unintelligible, owing principally to some conventional terms introduced in it; but after having read the book, its meaning became clearer.

It is impossible, in the short space of a review, to give a satisfactory account of an almost metaphysical work, for the clear understanding of which, the paragraphs which *precede*

are almost always necessary for the understanding of those which *follow*, but we shall endeavour to give something of an outline of Dr. Freke's views:—

"Seek out" (he writes) "for one of the simplest and least complicated germs to be found in organic creation; ascertain the nature of its constitution, observe the nature of its function, and I feel assured we shall therein have discovered the nature, the constitution, and the function of every embryonic germ of the entire organic creation. . . . . Take the germ of some individual of the family of the fungi, and what do we find? We find a single grain, as it were, of dust, which when shed upon the *inorganic* earth, we see developing or generating a fungus identical with that which the germ has left."

The function, therefore, of the *germs* is to confer organization, and as such Dr. Freke terms them in his book "organizing agents," but their duty is twofold; the lowest germ, for instance, in the vegetable world, not only has to convert oxygen, hydrogen, nitrogen, and carbon, into an organic atom, similar to itself, but by its death, which is a necessary consequence of its having imparted organization to mineral matter, it gives rise to what Dr. Freke terms an "*organic residual product*," which serves as *food*, or *specific stimulus*, to an atom higher in the scale of organization, in the same way as the inorganic compound of oxygen, hydrogen, nitrogen, and carbon had been the food or the specific stimulus of the germ of lowest organization; and so on, each successive elevation or development serves as pabulum or food to a higher organizing atom, until eventually we come to the highest of all, the organizing atoms of muscular fibre, nervous tissue, cerebral matter, &c., which substances are the *residual products* of their corresponding *organizing atoms*—these residual products by their death giving rise to the phenomena of animal life, mental phenomena, muscular phenomena, &c., and in doing so revert eventually to the four original elementary substances, oxygen, hydrogen, nitrogen, and carbon, which again, serving as specific stimulus to a germ of the lowest order, re-enter the great and endless chain of organic existence.

The lowest vegetable germ is, according to Dr. Freke, a *simple indivisible physiological atom*; as we rise in the scale of organization, the germs become more complicated, till at last in man the Graäffian vesicle is the most complex of all. It is in the diversity of the arrangement of the composing atoms of the several germs of the different plants and animals which inhabit this planet, that Dr. Freke recognises the diversity of

species, a diversity which must have existed from the commencement of time, when the great Architect of the universe clothed this earth with verdure, and commanded it to "bring forth the living creature after its kind, the cattle and the creeping thing."

In answer to the question of how did those complex germs arise, Dr. Freke tells us it was by a law of organic affinity, implanted in them *ab initio*, similar to the corresponding law in the mineral kingdom, and by which they congregated into varius groups.

The difference between vegetable and animal life, according to him, is both simple and new. In vegetable life, each atom in reproducing a number of its own species *dies*, at the same time that it gives origin to a "residual organized product," which may go on accumulating almost *ad infinitum*, as for instance in trees, in some of which the successive layers of wood can be counted back for centuries of existence; for in vegetable life the only death required is that of the organizing atom; in animal life, not only does the organizing atom die, but also the organized residual product, which as muscle, or brain, or nerve, dies away, and reverts to its primeval inorganic components, in giving birth to its respective muscular, mental, or nervous phenomena.

We hope that Dr. Freke will continue to investigate this subject; and in the mean time we trust that physiologists will look into this little book, and not turn from it, if after the first few pages they should find its diction too obscure, or its matter difficult to understand.

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*Vestiges of the Natural History of Creation.* Eleventh Edition. London: Churchill. 1860. 8vo, pp. 286 and lxiv.

This is now the eleventh edition of a foundling work, the unclaimed offspring of a wonderfully clever imagination, beautifully written, full of interesting matter, most fascinatingly detailed, but, alas, not always true!

We regret that this eleventh edition should be so much inferior to the previous one, both in the paper and the print; the preface also has been omitted, which we look upon as a serious mistake.

An attempt has been made to bring the geological portion of it up to the present mark of scientific discovery, and at page 100 we read:—

"Since the above was written, English geologists have accepted a series of facts implying the existence of the human race at a stage of the Drift era, prior to the extinction of the animals of the bone-caves. At various depths, up to twenty feet, in the naturally-disposed chalk gravel, near the banks of the Somme, in Picardy, have been found multitudes of flint implements, rudely chiselled by the human hand, and apparently designed to serve as spear-heads. They obviously infer that a primitive savage people resided here before the deposition of this chalk gravel, an event of vast but unknown antiquity. To the same purport has been the discovery of similar flint implements in certain bone-caves, both in Sicily and England."

This work has been now for so many years before the public, and has been so often reviewed, that we shall not add another word either in its praise or censure, but shall close our review by a few quotations, which have some bearing on the subsequent labours of Dr. Freke—whose work we have been just reviewing—and who has endeavoured to elaborate the previous conceptions of the author of the Vestiges. At page 105 we find the following words:—

"The whole aim of science from the beginning has been to ascertain law; one set of phenomena after another has been brought under this conception, without one ever feeling that God was less the adorable Creator of his own world."

And also that—

"It is not easy to say what is presumed to be the mode of His operations. The ignorant believe the very hand of the Deity to be at work. Among the learned we hear of creative fiats, interferences, interpositions of creative energies, all very obscure phrases, apparently not susceptible of scientific explanation. . . . . let the contrast between the two propositions be well marked. According to the first, all is done by the continuous energy of the Divine will—a power which has no regard to great or small. According to the second, there is a procedure strictly resembling that of a human being in the management of his affairs."

But God is the same yesterday, to-day, and to-morrow; and the same fires which upheaved continents in the early ages of our planet, are still working at present, upheaving the western shores of the South American continent; depressing the shores of Italy along the coast of Pozzuoli; and—

"in the ripple-mark on the sandy beaches of the present day, we see Nature's exact repetition of the operations by which she impressed similar features on the sandstones of the Carboniferous era. Even such marks as wind-slanted rain would produce in our day

on tide-deserted sands, have been read upon tablets of the ancient strata. It is the same Nature—that is to say, God through or in the manner of Nature—working every where and in all time, causing the wind to blow, and the rain to fall, and the tide to ebb and flow, inconceivable ages before the birth of our race, as now. So also we learn from the conifers of those old ages that there were winter and summer upon earth, before any of us lived to liken the one to all that is genial in our own nature, or to say that the other breathed no airs so unkind as man's ingratitude."

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*Quarantine and Quarantine Laws.—Return to an Address of the Honourable the House of Commons, dated May 22nd,* 1860, *for "Copy of Abstract of Regulations in force in Foreign Countries respecting Quarantine, communicated to the Board of Trade."* Blue Book.

*Return to an Order of the Honourable the House of Commons, dated 18th August,* 1860, *for "Abstracts of Returns of Information on the Laws of Quarantine which have been obtained by the Board of Trade."* Blue Book.

The two papers, the titles of which stand at the head of this notice, contain a vast amount of interesting information regarding the quarantine laws, which has been obtained and prepared from official returns by the "Quarantine Committee of the National Association for the promotion of Social Science." In fulfilling this arduous task, the Quarantine Committee have performed a most valuable labour, having alike done good service to the interests of commerce and to the cause of sanitary science.

A more oppressive, vexatious, and at the same time absurd system of laws could not be contrived than that which has usually prevailed throughout the world, under the name of "quarantine laws." Based upon false principles, administered often in a partial, selfish, and ungenerous spirit, and too frequently casting aside the sacred dictates of humanity, how could these laws be other than cruel and oppressive to those who come under their operation? But, beyond all this, the restrictions which they placed, or still place, upon commerce, the false security which they give to the places where they are most stringently enforced, with a consequent neglect of all true sanitary operations, are calculated to produce, and have produced, most injurious effects upon the communities for whose protection they have been devised. The Returns before us fully, and in hundreds of instances, establish these statements.

There is scarcely a page from which we might not quote conclusive evidence regarding these several points; but a few examples will suffice, and we shall content ourselves by laying before our readers some extracts from "Papers relating to Quarantine Laws," and from "Papers relating to Quarantine Practice;" by which we believe the whole existing system will appear to be one of unmitigated evil,—full of fallacy, unsound, and untenable. We imagine, indeed, that in this country there is not much need to argue the question; nor shall we do so, beyond making some comments upon the evidence, as it turns up everywhere throughout the Returns now under review.

In Spain and Portugal, two countries notoriously negligent of the laws of sanitary science, the laws of quarantine are excessively stringent, unequal, and unjust.

Mr. Consul Mark, at Malaga, says:—

"Arrivals from Egypt with raw cotton are admitted to pratique after eight or ten days' voyage, while arrivals from England with a cargo of coals, and after a passage of twenty-five to forty or fifty days, are all quarantined for three days."

Again:—

"In 1853 and in 1854, the quarantine against Great Britain and other countries where the cholera existed, was most stringent in all the ports of Spain. In 1854, the pestilence, nevertheless, raged throughout the Peninsula."

Again:—

"Mr. Consul Mark, alluding to the evils, inconsistencies, and glaring absurdities of the system of Spain, as practised at Malaga, states:—'Under the specious pretext of preservation of the public health, our trade with Spain is subjected to a series of harassing restrictions and taxation, from which the commerce of other nations is exempt. It is mortifying to see vessels from healthy British ports forced to perform quarantine in the infected ports of this country, and to find arrivals from the British Isles suffering three days' observation all the year round.

"'The evils of quarantine here are still further increased by the absence of all system, or unity of action, among the provincial Boards of Health; the law is interpreted according to the fancy of each Junta. The provincial Boards have repeatedly acted, each of them upon their own judgment, and in contradiction to the supreme Junta at Madrid. No injury to the health of the country seems to have followed, however vexatious to commerce.'"

In Portugal we find that "All vessels coming from the Brazils and the West Indies are more or less subject to qua-

rantine throughout the year. This quarantine is regulated by the Board"—the Board of Health at Lisbon, over which the Government has no control—"and is dependent upon the information received from the Portuguese Consuls at the ports of departure." "Arrivals from a port infected with yellow fever have to perform a quarantine of eight days; from a port suspected of the same, a quarantine of five days:" and this, too, although the ship's company are quite healthy! Nearly similar rules apply to cholera; and "These different periods of quarantine are irrespective of the length of the voyage, whether this has been only a week, or two months and upwards; and it would seem that they are very generally enforced." "At the beginning of this year, 1860, the whole coast of Brazil was declared infected with yellow fever, in consequence of its existence in one port; and a vessel which arrived from the port, Para, with sickness on board, was ordered 'to quit the port,' Lisbon, 'or *be sunk*, and the susceptible portion of the cargo now in the lazaretto to be destroyed!'" Could there be anything more monstrous and absurd? Spain and Portugal, two of the dirtiest countries in Europe, admitting an arrival from the filthiest place in the world to free pratique, after an eight or ten days' voyage; while a quarantine of three days is enforced upon ships from England, though they had been at sea from twenty-five to fifty days!

As might be expected, the quarantine laws in force in the kingdom of the two Sicilies are or were "minutely elaborate and extremely rigorous,—more so, probably, than in any other country." In fact, in countries low in the scale of civilization, and under a tyrannous government, such a state of things naturally exists; but it is now to be hoped that a more enlightened system will prevail, and that, as in Sardinia in 1852, a great reform will take place in the quarantine laws. In fact, we believe that all thinking people will see the necessity for a marked modification of a system that has been proved, over and over again, to have been, not only perfectly useless, but even pernicious in its results. Strict quarantine has been enforced, and "sanitary" cordons, as they were miscalled, have been established when Cholera has prevailed, but "with the most signal want of success;" and we perfectly agree with the following views, thus stated by Consul Hunt:—

"After a residence of twelve years in extreme climates, the result of my experience is, that the epidemic diseases for which quarantine is usually imposed are exclusively the result of local atmospheric derangements, and that the spread of these diseases can in no case be prevented by the infliction of quarantine."

Moreover, by a reference to the Returns now before us we find, that in several states where the strictest quarantine had been enforced against cholera, but without success in excluding the disease, the rigour of the system has been greatly lessened, or quarantine has been abandoned; thus exhibiting a growing disbelief in the protection which quarantine had been deemed to afford. Then, again, we have in these Returns conclusive evidence that the necessity for quarantine has not existed, in almost any instance; for, in the lazarets, where passengers, crews, and goods have been landed, the disease against which the quarantine had been directed has scarcely ever appeared among the officials. For instance, take Lisbon, where the most severe quarantine has been enforced.—"The only cases of epidemic disease," says Dr. Donnett, R. N., "that have appeared in the lazarets for the last forty-three years, are three cases of cholera, in July, 1856, whilst the disease was raging in Lisbon." Similar evidence might be quoted from the records of almost every other lazaret, showing clearly that a system so pregnant with injurious restrictions on commerce should not have been enforced, and should no longer be tolerated. Plague, yellow fever, cholera, and small-pox, are the diseases against which the most stringent regulations have been adopted; yet the mass of evidence in these Returns shows the fallacy of the quarantine laws. In some instances these have been enforced against a disease supposed to be introduced, or coming from without, whilst it was raging within, fomented and cherished by the very system which was deemed to afford protection—for example, the last epidemic of yellow fever in Lisbon. In other cases we find nearly all the world restricted in its intercommunication in consequence of *reports* of disease existing in some out-of-the-way place, or from the prevalence of fever in a remote and filthy Moorish town!

It is much to be regretted that out of the learned International Quarantine Conference held at Paris in 1851–52, something more usefully determinate regarding the quarantine laws has not arisen; for, while the commission admitted the necessity for sanitary operations, properly so called, no abatement of the oppressive system of quarantine in force was proposed. Instead of regarding the subject in its true aspect, and recommending every country to attend to its own internal sanitary condition, as the only certain means of protection from disease, the Convention seems to have admitted this merely as a truth, but retained quarantine as a necessity. "It is satisfactory, notwithstanding the retention of such rigorous practices, to know that all the delegates of the Convention, with the exception only of

the Papal and Neapolitan, confirmed the important result of Sir W. Pym's observations in 1844, that not an authenticated instance of the plague has occurred in any lazaretto in the Mediterranean among all the men engaged in handling the cargoes of infected or suspected vessels, or of a case of the disease having ever been known to be introduced into a country by cotton bales, or other similar goods."

Now, like evidence is abundantly afforded by the Returns as regards yellow fever and cholera also. Hence, surely, the undefined bugbear of contagion should not be permitted longer to restrict commerce and cripple sanitary science. It is time that every country should awake from this dream of supposed security, and look to its internal condition, not imagining that epidemic disease comes from without, and can be kept out by any system of quarantine or cordons, but acting rather under the belief that the removal of filth, the causes of malaria,—in a word, the known impediments to health, are the only sure means of preventing disease from taking its rise in, or spreading through a community. The rapid intercommunication, indeed, which now exists through the agency of steam, must soon lead to the breaking down of the established quarantine laws, and the enactment of others more consonant with justice and common sense, and more compatible with the interests of trade. In aiding this most desirable consummation, these Returns must hold a prominent place, as they exhibit to the most prejudiced and timid, data which should be conclusive regarding the fallacy, the inequality, injustice, and, in some respects, the inhumanity of the recognized method of quarantine. But, beyond this, these Returns also in the most decided manner make reference to the marked influence which neglect of sanitary operations exercised everywhere in the spread of epidemic disease, and also satisfactorily show the salutary effects produced by them whenever properly carried out. In fact, the Reports furnished by authority from almost every part of the world, give clear evidence upon these points; and, although several of them seem still to favour the imposition of strict quarantine, they all agree that both on board ship and on shore much greater attention than has hitherto been observed should be paid to the causes which affect health, and either induce or propagate disease.

For our own part, we believe that were a stricter attention given to sanitary science—were its salutary principles diffused, generally received, and acted on, the spread of disease would be very much limited, and its ravages greatly lessened; and hence we also believe that the quarantine laws could be with

perfect safety either totally abolished, or so much modified that they would not, as at present, injuriously affect commerce, and be a vexatious nuisance in many parts of the world. Whether all civilized countries will soon be led to adopt these views, it is impossible to determine. We know that it is difficult to overcome old prejudices and ancient customs—still more difficult to do away with systems in the maintenance of which many officials have a direct personal interest; yet we are perfectly satisfied that the spread of sanitary knowledge, the increase of national intercommunication, and the observation of the inefficiency of quarantine regulations will, at no very distant date, lead to a generally uniform, liberal, and simple system of quarantine, if any such system should still be considered judicious or desirable. And, as we have already said, the Returns now under notice will tend greatly to open up the question, and to diffuse sound knowledge regarding a subject hitherto but little understood. No one can rise from carefully perusing the evidence they afford, without being deeply impressed by the important information they contain, and satisfied also of the candid spirit in which the Quarantine Committee of the National Association for the promotion of Social Science have made abstracts of these returns. The labour of examining the vast amount of documentary evidence, and of making these condensed but clear Reports, must have been very great; but we feel certain the gentlemen who undertook this arduous task will have the gratification of knowing that their work has not been in vain. We regard it as one of those philanthropic undertakings in which this country has so often been the pioneer, and which are of such social importance to the human family. The spread of sanitary science, the inculcation of sound sanitary principles, is a work of vast importance; and every man, every association of men, who aids its progress must be regarded as filling the place of a public benefactor; for, what undertaking can be more beneficent than an inquiry into those laws which affect the social condition of man, and with which his health and happiness are so closely allied?

*Manuel de Médecine Operatorie.* Par J. F. Malgaigne, Professeur de Médecine Operatoire à la Faculté de Médecine de Paris. 1860. 12mo.

M. Malgaigne has, in the work before us, endeavoured to do for the French students what Mr. Fergusson had already done for ours in his "System of Practical Surgery." The book is the same size, the same thickness, and has even the same number of pages; but it lacks the beautiful illustrations which add so much to the value of our English publication, at the same time that it is deficient of much valuable information. The print is also considerably larger; and therefore what is, perhaps, gained in clearness of text, is lost in subject-matter. The first fifty-six pages are devoted to the consideration of the more elementary portions of operative surgery; the description of instruments, and their uses; the application of caustics, and the several modes of suppressing hemorrhage; terminating with a short chapter on etherization, in which he warmly advocates the use of chloroform, asserting that in his hands it has never failed to produce the desired effect; and that he never had the misfortune of meeting with an accident. He applies it simply on a folded cloth, taking care to allow the patient plenty of air to breathe at the same time. Speaking of the écraseur, he says "Although I do not admit all M. Chassaignac's assertions concerning it, yet it appears certain that, if carefully applied, it will effect the division of tissues without hemorrhage, at least when they are traversed by arteries of moderate caliber. I doubt, however, its becoming universally employed --first, in consequence of its complicated nature; and, also, from the danger of its breaking during an operation, as I have seen happen." The fourth chapter of this introductory portion is devoted to the consideration of cauteries, potential and actual. The hot iron, which once used to be the surgeon's sceptre, has fallen too much into disuse in these countries; not so, however, on the Continent, where it is still much employed. There can be no doubt that, under judicious management, it is a most useful agent for good.

While speaking of abscesses, Malgaigne recommends that such as happen about the face should be allowed to break of themselves, "to prevent the formation of cicatrices." But it appears to us, that to allow an abscess to ulcerate its way to the surface would be about the best way of ensuring a well-puckered cicatrix, which might be avoided by an early incision while the skin was still sound.

The article on transfusion of blood is interesting, though

too concise, which is, indeed, the main error of this Manual. M. Malgaigne remarks, that although J. Hunter, Scudamore, and J. Davy, have fully demonstrated that blood coagulates sooner in heat than in cold, still directions continue to be given in systematic works on surgery to keep the blood warm during the process of transfusion, in order to prevent its coagulation.

The article on aneurisms is also much too brief. In it the author advocates compression in preference to deligation, although, from statistics gathered by himself, he found 70 cures out of 108 popliteal aneurisms treated by ligature, and only 61 cures out of 108 similar cases treated by compression. He is of opinion, that the best treatment will consist in diminishing the current of blood into the aneurism for the first few hours, and then, by increased pressure, stopping it altogether. He also recommends to apply a certain amount of pressure on the distal side of the aneurism, to prevent, if possible, the reflux of blood into the sac from the collateral circulation, to which reflux the majority of failures is due.

In describing the operations for tying the arteries of the lower limbs, he is so extremely laconic, that he is very nearly useless to the student; and in his account of the several modes of taking up the femoral artery in its upper-third, he completely ignores Dr. Porter's plan of a tranverse incision, by which the vessel can be tied much higher than by the longitudinal section, at the same time that it greatly facilitates the finding of the artery. Malgaigne's account of the operation for the removal of the upper jaw is also most unsatisfactory. After detailing, at greater length than usual, Gensoul's mode of proceeding, he merely mentions that Fergusson performs this operation with a single incision through the soft parts, without adding a word of the many improvements introduced since the days of Gensoul, to whom, certainly, is due the merit of having first executed this formidable operation; and the student who learns his operative surgery from Malgaigne's Manual would proceed to this operation encumbered with chain-saws, chisels, and mallet; and would disfigure his patient (should he even succeed in the operation) by leaving on his face the traces of three incisions, instead of one. As for the mode of securing the patient, or as to the propriety of supplying him with wine during the operation itself, or whether chloroform is admissible under such circumstances, he does not say a single word to guide the young practitioner. While on the subject of amputations, the author gives us some interesting statistics, which must be very gratifying to us; for in-

stance, he says that in Paris there are 50 per cent. of deaths after *non-traumatic* or pathologic amputations, while in London the mortality is only 22 per cent. Amputations of the thigh show a mortality in Paris of 60 per cent., and in London only 24 per cent.

Speaking of amputation through the wrist-joint or radio-carpal dis-articulation (an operation which is rarely, if ever, performed in these countries), he says, that he much prefers it to amputation of the fore-arm; first, because it leaves a longer stump, which is a fact of much importance to the working man; second, because, according to him, the mortality is much less than in amputation of the fore-arm. Out of nineteen cases collected in the hospitals of Paris, he counted eight deaths; whereas, in sixteen cases of radio-carpal dis-articulation, not one death could be enumerated. If these statistics could be relied on, they would be quite sufficient to cause the adoption of the latter operation in preference to amputation above the wrist; but we think there are still further reasons to recommend the adoption of this operation in appropriate cases, viz., the greater length of the stump, together with the retained power of pronation and supination. Liston, one of the greatest surgeons of modern times, says—"Amputation of the fore-arm should not be attempted *below its middle*, otherwise a good covering cannot be had for the ends of the bones." Fergusson, of King's College, says of this operation, radio-carpal dis-articulation, "Surgeons in this country have had but little experience of it. . . . . I should certainly wish to see the practice more frequently tried than has yet been the case." He adds, further down, "I should certainly cut off the articular surface of the radius, if a saw or forceps were by me at the time." Malgaigne, however, does not say a word about removing the articular surfaces in this operation more than in amputation through the elbow; and yet, he gives the most encouraging statistics for both, and concludes his article on the latter operation by saying, "la supériorité de cette opération sur l'amputation du bras est donc désormais hors de question." Here, again, Malgaigne is at variance with Professor Fergusson, who states, "Notwithstanding the sanction of so high an authority as Dupuytren, who performed this operation frequently, it appears that there are few advocates for it in the present day; and, for my own part, if I did perform it, I should most certainly cut off the articular surface of the humerus." If Malgaigne's statistics are to be believed, and we can see no reasons for impugning their accuracy, the plan of amputating through the wrist and elbow-joint should receive more attention in

these countries than has been given to it; and certainly a fair trial to the operation, without resection of the articular end, should be given it, because every plan which tends to simplify a proceeding, by diminishing the number of instruments required, is undoubtedly a valuable improvement.

The next operation we come to is one which we trust never to see performed in this country—paracentesis of the pericardium. We must, however, do the author justice, by saying he does not in any way recommend it, at the same time that he does not condemn it. It appears to have been performed in France by no less than seven different surgeons, including Desault, Larrey, Jobert, Trousseau, and Aran. Different intercostal spaces were selected by each; and Aran even injected a solution of iodine into the pericardium of his unfortunate patient, adding, "et le malade ne parait pas s'en être mal trouvé;" though, from the absolute silence as to the result, we take it for granted, that although the patient did not feel very ill after the operation, yet the case ended in death.

On the whole, this Manual is so very much inferior in every way to all our standard works on surgery, that, although it may prove a boon to the French student, it would never repay an English one the trouble of reading it.

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*Clinical Memoirs on Abdominal Tumours and Intumescence.* By the late DR. BRIGHT. Reprinted from "Guy's Hospital Reports." Edited by G. HILARO BARLOW, M. D., M. A., Cantab. The New Sydenham Society, 1860. 8vo. pp. 326.

THE New Sydenham Society have done well in presenting to their readers this collection of essays by the late lamented and distinguished writer, Richard Bright. Few men in their generation have done more for their fellow-man than the late Dr. Bright; painstaking, accurate, conscientious, endowed with rare powers of observation, and gifted with a facile pen and high descriptive powers, all that he has ever written is endowed by these qualities with a high value in the estimation of his professional brethren; and we are of opinion that his editor, Dr. Barlow, has acted with a sound discretion in confining himself to the simple task of reprinting the essays as they appeared from the author's pen in the Guy's Hospital Reports. That he is not ignorant of the progress that medical science has made in some of the subjects treated of in these essays, is evidenced by the following passage that we extract from

the preface; while, at the same time, the argument put forward for not tampering with them, meets with our cordial concurrence:—

"The papers in question being essentially clinical, and consisting chiefly of well-grouped examples of the diseases under consideration, it has been considered better to reprint them almost exactly as they were originally published. The memoirs themselves had no pretension to constitute a complete monograph on the subjects which they illustrate. To have attempted to make them such by the addition of notes or interpolated material from other sources, would have been to wholly change the character of the work. It would have taken it beyond the scope of what was contemplated by the Council of the Society, and would also in some degree have deprived it of its greatest merit—that, namely, of being clinical portraits, fresh from the hand of a master. The task of editing has, therefore, as far as the body of the work is concerned, been restricted to the careful correction of verbal errors and obscurities of expression, and the re-arrangement of the plates, so as to bring them into juxtaposition with the cases to which they belong. In the original papers most of the illustrations were on stone, and were appended at the end of the volume; in the present reprint they have been reproduced in wood (by Mr. Tuffen West), and are incorporated with the text, so as to facilitate reference. The addition of the List of Cases, which is given at page xii., and of the Index, at page 323, will doubtless be found useful.

"In a few subjects, such, for example, as the histology of the acephalocyst hydatid, great advance in knowledge has been made since the publication of Dr. Bright's papers. It has been thought best, however, even in these matters, to adhere to the rule stated above, of not making any additions to the original statements."

Many of our readers, doubtless, will remember the substance of these essays, as they appeared in the Guy's Hospital Reports; to them their re-appearance in a collected form will be now acceptable; to such, however, who may not either remember them, or who have not before seen them, we may be permitted briefly to indicate the nature of their principal contents.

Dr. Bright's object was to bring his vast clinical experience and great diagnostic tact to bear on the elucidation of confessedly a most obscure department of medical disease—the discrimination and diagnosis of abdominal tumours; and this he has done by briefly stating the principal characteristics of these, as they are produced, either by the presence of tumours dependent on acephalocyst hydatids, by ovarian tumours, or diseases of the spleen, liver, or kidney; under each of these heads, we have valuable features recorded, by which in life they may be recognised; whilst after death their pathological

characters are described in a manner that leaves but one impression on our minds, that here indeed the author has held up the mirror to nature; and under each section we have a perfect *embarras de richesse*, in the shape of illustrative cases. The whole work is profusely filled with woodcuts and outlines descriptive of the several diseases described, by which means the author's verbal descriptions are more vividly presented to the reader's understanding. In the first chapter, which is devoted to "the exploration of the abdomen," we find a faithful description of the contents of this cavity, as they should be *in situ* in the healthy individual; and it concludes with a passage of such practical value, that we are tempted to reproduce it here for the benefit of our readers:—

"With a view of assisting in registering facts, it appears very desirable that every one who is really anxious to make the most of the experience which comes within his reach—a duty which, unfortunately, from the time it occupies, we are all too apt to neglect—should provide himself with some ready mode of transferring to the corner or the blank page of his note-book an outline of the abdomen; upon which he may mark, as nearly as possible, the exact position of any tumour which he is called upon to treat: and, for this purpose, I have employed one or two different little contrivances, which it may not be amiss to mention. In the first place, having drawn on a thick sheet of paper the outline desired, we may, with a pin, make holes in a few prominent points; and pricking the note-book through these holes, the least-experienced draftsman will be enabled to make an intelligible sketch in a very short time. I have likewise had the figure cut in a brass plate, to use it in the mode of stencilling; and have thus procured, in a few seconds, upon any part of the page, such an outline as is represented in Figs. 3 and 4. Again, it would be a matter of a very few shillings' expense, to have a woodcut or type formed, which might be used like a seal, even with common ink. It is obvious that no one single sketch can serve for every case; because the relative proportions of the different parts of the abdomen are somewhat altered, as it becomes distended, and consequently thrown out of its natural form; but still, the convenience of some such mechanical contrivance is very great, and there is no difficulty in being provided with more than one form of outline; and perhaps a second, representing the moderately distended abdomen, would be quite sufficient for every purpose. It will be at once perceived, by a reference to the figures, how the situation and extent of tumours, whether visible to the eye or ascertained by the touch, may be traced on the outline; and thus remain a fixed record, by which to judge of the progress of the individual case, or afford a means of comparison with others."

In conclusion, we have to congratulate the New Sydenham Society on the creditable manner in which these memoirs have

been produced, and their editor on the able manner in which he has discharged his task. In these days of publishing mania, it is pleasant to find parties that think of *republishing* standard works, such as these; and we only trust to see this spirit more widely diffused. At the moment the thought forces itself on our minds how gratefully we should feel to any one who would bethink himself of giving us another issue of that mine of wealth—our own old Dublin Hospital Reports—five volumes which, in our opinion, might challenge competition with any similar publication in any language, but which are now out of print, and consequently, almost out of the reach of our rising generation.

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*Over de Allantois en hare Vorming en Veranderingen in den Mensch.* Door J. L. C. Schroeder van der Kolk. Uitgegeven door de Koninklijke Akademie van Wetenschappen. Met eene Plaat. Amsterdam. C. G. van der Post. 1860. 4to, pp. 36.

*On the Allantois and its Development and Changes in the Human Subject.* By J. L. C. Schroeder van der Kolk. Published by the Royal Academy of Sciences. With a Plate.

The contributions to science of the distinguished author of the above essay are too numerous, important, and well known, to render it necessary for us to say a word in commendation of the manner in which the writer has on the present occasion performed his task. Suffice it to state that the same care and fidelity, and beauty of illustration in his admirable drawings, so conspicuous in the larger works which have from time to time emanated from his pen, are manifested in the paper on the Allantois. For details we must refer to the work itself, remarking only that among the most important points contained in it is the statement of the discovery that in the commencement the Allantois has a sort of villi, and that subsequently its sac separates from the stem and collapses, and that we thus have a guide to a good explanation of the abnormity known as the inverted bladder.

*A Treatise on Fever; or, Selections from a Course of Lectures on Fever.* Being part of a Course on the Theory and Practice of Medicine delivered by ROBERT D. LYONS, K. C. C., M. B. T. C. D., M. R. I. A., &c., &c.; Physician to Jervis-street Hospital; late Pathologist-in-Chief to the British Army in the Crimea, &c., &c. London: Longman. 1861. 8vo, pp. 473.

THE subject of fever must always command and obtain attention. It is emphatically in the art of medicine *the* subject, important in all its aspects, whether local or general, individual or social. It is the foe which carries on from the first an internecine and truceless war against the human race, pulling down the strength of the strong man to the helplessness of the child; and, in the touching language of England's great philosopher, "stripping the mind of its ideas, and in a few days calcining by its flames all those images to dust and confusion which seemed to be as lasting as if graved in marble." Too often we see it, at home with us, following in the footsteps of famine, sweeping away the wretched inmates of the peasant's hut whose debilitated frames offer but little resistance, or startling us by its appearance in the higher walks of life, reaping the summer corn before it is ripe. We see it again as the scourge of camps, more deadly than all those scientific agents together which ambition has invented for legitimate slaughter. As the "Kriegs-pest," or war-plague, we have read in our boyhood of its stealthy and unsuspected ravages in the Grecian camp, in that glowing Epic which describes the *earliest* siege on record; and in the *latest* which has occurred, we have painful evidence in the work before us, that medical science has not yet succeeded in disarming her terrible enemy.

Since the time of Hippocrates, much has been written on this disease; but, as regards its hidden nature, it still merits the name of "the pestilence which walketh in darkness." Yet this ignorance of ours does not proceed from any lack of the spirit of inquiry; our book-shelves labour under the great variety of speculations about its essence and operations. Indeed, as Judge Jeffreys once said of the writings of Richard Baxter, the bare theories of fever "would fill a cart." And it forms a curious illustration of the vanity of human expectations, that in the numerous theoretical works on this subject, it is precisely the portion most elaborately wrought which soonest falls into disesteem. The valuable practical facts have been carefully stored up from age to age, while the theories have been winnowed away. Some pathologist proposes to the world an in-

genious hypothesis, which, he thinks, appropriately colligates the facts, and this holds good until experience shows that the bond is fragile. Still we should not discourage such researches because they are conjectural. It is said that the famous Kepler made nineteen guesses before he discovered the true planetary curve.

The work on fever now before us consists of *excerpta* from a course of Lectures on the Theory and Practice of Medicine addressed by the author to his class. He gives therein the results of a large experience in the observation of fever, beginning with Ireland. Unhappily, this country possesses the unenviable distinction of being the land of fever; as the author graphically describes it:—"Typhus, chief habitat, Ireland." Can it be that the Gælic branch of the Celtic race possesses a constitutional susceptibility to this disease more than the Cymric variety, and much more than the Anglo-Saxon? Or are we to look to a plurality of causes, indigenous to Ireland, but less prevalent elsewhere, for an explanation of the phenomenon? In the terrible famine fever of 1847 and subsequent years, the author, in common with his brethren over the length and breadth of the land, had the sad privilege of studying it in its most characteristic form. He had also, by his appointment as Pathologist-in chief to the British Army in the Crimea, possessed many facilities for studying the "bellica febris" or war-typhus, both in the camp before Sebastopol, and in the hospitals at Scutari. Lastly, the author visited Lisbon, in 1857, to investigate the epidemic of yellow fever prevalent there, and has furnished us with important records, taken on the spot, of the pathological anatomy of this group of fevers. He is thus enabled from personal observation to sketch vividly the lineaments of these different, yet similar forms of disease.

"Facies non omnibus una,
Nec diversa tamen, qualem decet esse sororum."

From such opportunities, duly appreciated, we should expect both valuable and original matter; and after a careful perusal of his book, we are not disappointed.

The present work comprises eleven chapters, of which the first two are devoted to general observations on fever and its pathology; the third, to the classification of its specific forms, which are reduced under three great types, viz., Primary, Irritative, and Eruptive; the five next are taken up with a description of simple continued fever, the varieties of synochal fever typhus, typhoid, and the fever of the Crimea, which he regards as the typhoid form. Having concluded the subject of con-

tinued fevers, he should next, according to his classification, have taken up *seriatim* the intermittent and remittent subdivisions of primary fevers; but he passes over the first in a few pages, as not coming within the scope of his work, and discusses in the closing chapters Yellow Fever as a branch of the paludal form. It will thus be seen that Dr. Lyons does not profess to treat of fever in its widest signification; for example, of the above-mentioned three leading groups, he does not attempt the two last at all viz., the irritative and eruptive; and but partially the two orders of the class primary. We have here ventured to reverse the nomenclature of the author, who, in the heading of the ninth chapter, has seemingly by a technical oversight written thus—"Order 1. Primary Fevers; Class III.—Remittent Fevers,"—as his *subsumption* (as logicians call it) of *class*, as the less general expression, under *order* as the more general, is opposed to the use of these terms by the best writers on natural history.

With respect to the pathology of fever, it is almost universally admitted that the great central phenomena of this disease to which the others converge, is the production of animal heat above the normal standard. It is curious how closely general opinion here coincides with scientific research. We trace the one striking condition in the ancient epithets which have been applied to this disease; thus, the causus of Aretæus, the πυρετος of the Greeks, the febris[a] of the Latins, were names all borrowed from sensible images expressing heat. This morbid calorification is, of all the symptoms of fever, the only one which is never absent in some or other of its phases. Dr. Lyons has given his adhesion to the views of Virchow, of Berlin, which are based on this foundation—

"Though," he says, "I am far from saying that they are to be considered as giving us a whole and complete solution of the pathological problem, what is fever? they may, I think be safely accepted as embodying a large amount of what is most essential for the ultimate determination of this question."

In what may be called the *common-sense* view of fever there are three elements associated together, namely, increase of temperature, frequency of circulation, and general wasting of the body. These form the fundamental positions of Virchow, but he endeavours to make the chain complete by additional links. We may state his views from the very clear outline of them

[a] We cannot agree with Dr. Copland, who derives febris from "ferbeo, or februo, I cleanse;" it is, as Varro observes, from ferveo, to be hot,—fervor, heat.

given by Dr. Lyons, briefly as follows.—He assumes, not without reason, that the morbid production of animal heat is the *essence* of fevers. The fact, at least, that there is such an increase of temperature, is undeniable as not only the unaided senses show it, but the thermometer also indicates a rise of several degrees above the natural standard in the interior of the body. Where, then, are we to seek for the cause of this augmented temperature? Is it in the nervous system, the mass of the blood, the respiratory function, or the nutrition of tissues, that the *point de depart* begins? In the present state of medical science we cannot speak with certainty. But, whether the elevation of temperature arises from a single cause or a plurality of causes, the mass of the blood, pervading as it does all parts of the system, comes very soon to share in it. Looking to analogy, we should be led to seek for the elevated temperature of the febrile state "in an increased activity of the causes which operate in the production of the physiological or healthy temperature;" and as we know that the normal production of heat is satisfactorily explained by changes in the nutrition of tissues, it is fair to conclude, by a parity of reasoning, that abnormal heat should result from abnormal changes in the tissues. This is an important step in advance. Virchow connects increased heat with increased consumption of organic matter; the mysterious chemical apparatus going on within our bodies is disordered; it wants fuel, and, failing to obtain materials from without, it uses up the elements within, both fluids and solids undergoing metamorphic changes to a remarkable extent, as evidenced in the wasting which goes on *pari passu* in the outer parts of the body. We have now to connect the above phenomena with disordered innervation.

"Taking into account," says Dr. Lyons, "the various phenomena which attend the onset of fever, characterised as it is by weakness, prostration, and loss of energy, especially in the muscles and sensitive apparatus . . . we may conclude with Virchow that the elevation of temperature which is found to be amongst the earliest as well as most constant of the symptoms, is a *paralytic* phenomena. Though not identical with the ordinary paralytic state it is in all essential respects analogous to it, and can only be produced by a loss of power in the nerves which constitute the natural regulators, or it may be moderators, of the development of animal heat."

There are some important arguments founded on experiments in support of the preceding views, rendering it probable that, as Cullen long ago maintained, the first step in the establishment of febrile action is to be sought for in the nervous system. Upon such a supposition, the order of the phenomena

would be this, viz.:—nervous disorder, increase of circulation, augmented nutrient metamorphosis of tissue, elevation of animal heat. We would thus have a chain of sequences, of which the last can be affirmed of the first. How then, it may be asked, are we to suppose that the nerves can regulate the animal temperature? By the intervention of the vascular system over which the nervous system exercises, by the vasimotor nerves and in other ways, a great influence. Here experiment steps in to our aid. It renders probable the supposition that in the animal body the development of heat in the normal state is kept in bounds by certain nerves; and that in fever, when this curbing power is enfeebled, the heat rises. Thus Bernard's experiment of dividing the sympathetic nerve in the neck was followed by a rise of temperature in the corresponding half of the head. Dr. Lyons has brought an interesting body of evidence to bear on the same point; and he renders it highly probable "that the vagi nerves, and we would add the sympathetic, are primarily, if not chiefly engaged in the production of the febrile phenomena."

But we have one step more to advance: we trace back the vagus nerve to its origin, and we there find the centre of this regulating property, which in Virchow's opinion resides in the medulla oblongata, near the roots of the vagi.

To sum up the preceding remarks—the weight of the fever poison would seem to be primarily exerted upon those centres of the nervous system which regulate the consumption of the nutrient material, and to produce there a degree of irritation which tends to arrest the controlling power.

"As the power of the moderating centres is arrested by the abnormal tension or irritation, the consumption of material advances throughout the tissues, and in proportion the heat of the body increases, and the particular point of the commencement of fever is reached. At first we see only the weakening of the corporeal and mental powers, which follows directly on the condition of tension or irritation being induced; and this in some instances with such suddenness of the prostration of the vital powers as to suggest the idea of a true paralytic invasion of the nervous system."

We have already observed that Dr. Lyons reduces fever under the three great types of primary, irritative and eruptive. Acknowledging the imperfect state of our present knowledge of the etiology and general pathology of fever as an impediment to a classification upon a scientific basis, he states that "he considers the proposed arrangement as being free from grave practical objections, and at once simple, comprehensive,

and readily committed to memory." We fully coincide with the author upon the difficulty in question, and are of opinion that it is easier to criticise any given classification than to propose a better; still, as a good classification of any subject is a great aid both to accuracy of knowledge and to the memory, we are not in favour of the simplest arrangement, if it be not also the best. Taking pyrexia as a general characteristic of the three groups, the author makes the essential difference of primary fevers to consist in an absence of specific pathological lesion, "and as having no necessary connexion with localised disease in any portion of the body, or in any organ or tissue." The irritative fevers, he observes, have an anatomical seat, "and owe their existence to lesions of a well-defined kind in particular parts of the system." Again, the eruptive fevers are without lesion of the viscera, while the cutaneous surface is the seat of pathological changes. Now, it may be objected to this division, that granting the basis to be correct, (which in primary fevers is not so generally admitted) the laws of division are not strictly observed, as the three groups are not sufficiently *opposed.* A legitimate division of fever upon the basis of the presence or absence of local lesion should be made primarily, it appears to us, into two groups ; viz. : those *without* any local lesion, primary fevers; and those with manifest lesion or change, viz.: irritative and eruptive fevers—the lesion in the two last differing in the *seat,* which is internal or visceral in the first, and external or cutaneous in the other. Whether the author has taken a proper ground of distinction in this test, is another matter altogether. Again, on his basis of classification, exanthematic typhus should be classed under eruptive fever; and though he makes the relation of their phases to *time* to be a sufficient ground for separating primary into continued, intermittent, and remittent, he is obliged to resign this element of distinction, and place one form of remittent under another group. But, while we make these remarks on the author's classification, we would be sorry not to do him the justice of observing that the subject admits of so many cross-divisions, it is difficult to group the forms under suitable heads without violating rule. The varieties of fever are so numerous, especially the climatic forms, that a table of nosology, in order to include them all, should be so large as to be cumbrous.

We need not dwell upon that form of inflammatory fever which has long been a puzzle to students under the name of synocha. It has had the ill fortune to be regarded as a myth, and to be rejected without scruple from the catalogue of fevers; and French writers have had the hardihood, according to Dr.

Lyons, to pronounce that all the synochal fevers from Hippocrates down are gastro-enteritic fevers in disguise. The author, however, stands up for its existence, and avers that he has seen well-marked examples of it in Southern and Eastern Europe. With respect to the scepticism about the frequency of its appearance in former times compared with the present, the author justly condemns the fallacy which assumes that because it is now rarely met with, it has never existed. While on this subject, we may notice an interesting remark of his upon the influence which peculiarities of diet seem to possess in modifying fever.

"I have myself formed the notion, but I in no way insist on it as a well-grounded hypothesis, that the sthenic or synochal types of fever are in the present day, and perhaps have always been, most remarkably developed amongst the graminivorous and herbivorous races of men; while the putrid or typhus types of febrile action more readily develope themselves in the races amongst which animal food constitutes a large part of their ordinary aliment."

If subsequent observation should prove the above conjecture well founded, and the geographical limits of typhus give it an air of probability, it would afford no small triumph to the vegetarians. Does it, however, we would suggest, consist with the spread of typhus among the peasantry of Ireland with whom a vegetable diet has always so widely prevailed?

We have read with great pleasure the judicious and practical observations which are made at the close of the chapter on inflammatory fever, and which show the reflecting physician. That plan of treatment which has been based on a powerful action upon some *one* system as a means of eliminating a disease extending through *every* system, is noticed by him only to be strongly condemned. The violent purgation once so common, while the Hamiltonian system was in vogue, has been found to set the fever astray from its natural course, and to produce a train of abdominal mischief. Excessive diaphoresis has been observed by the author to be similarly injurious in some cases, being followed by suppression of urine. Excessive depletion, general or local, is equally pernicious; and among the forms of bleeding occasionally required in fever, he unequivocally condemns the section of the temporal artery, for seemingly valid reasons. He objects to all exclusive modes of treatment, as repugnant to common sense; so that the system which meets his approval is the eclectic, no doubt, the most suited to fevers. Unfortunately, eclecticism is not always in the same favour with the public as with the profession,—particular systems, how-

ever incongruous, being held to be proofs of consummate genius by the former. Some one has said that "Eclecticism was like a coalition ministry," from which no good was to be expected, because there must be more or less compromise of individual opinions. However, in fever-therapeutics, except in the periodical forms of that disease, we possess as yet no specific either for their arrest or cure.

Relapsing fever has, we think scarcely met with adequate representation in the work before us, not more than half a dozen pages being specially devoted to it. In our former volumes, however, we have such extensive reports upon this form of fever, that we regard its clinical history as almost complete. The author takes exception to the generally received statement, "that relapsing fever constituted the large majority of the cases of famine fevers of Ireland," his experience in the late epidemic being "that the maculated typhus was the disease which chiefly prevailed, while the relapsing fever presented itself only at the close of the great typhus visitation." He argues that the great decennial mortality of 200,000 is too large to be accounted for by a fever so little fatal as the ordinary kind. To admit the force of the latter argument, we would require to know the numbers attacked with fever within the time, as the recoveries might have been to a large amount from relapsing fever, and the deaths from the severer forms. The opinion itself can only be affirmed or denied by the united experience of our medical brethren throughout Ireland.

The chapter on typhus commences with the adoption of the term typhosis, as an imaginary head, to which five forms are referred, as follows:—1. Typhus proper, comprising the spotted, petechial, malignant, and gaol or camp fever, and the Irish typhus *par excellence*. 2. Typhoid, or enteric, or dothinenterite, otherwise known as the Pythogenic,[a] and sometimes as the French or Continental fever. 3. Typhoid bronchitis, pneumonia, &c. 4. Cholera typhoid. 5. Traumatic typhoid. Of these five, the two first present the characters of independent fevers; whereas the pyrexial state in the three last seems to take its origin from localised disease.

From the observations of the author, it would seem that he

[a] This term needs explanation, and, in another part of the book, it is defined as that form of fever which is produced from animal decomposition. It is there derived from πυθων, rotting, and γίγνομαι, I beget. Whoever invented this word did not observe the genius of the language, for compounds of two verbs are not common in Greek. Further, as διογενης means sprung from Jove, so πυθωγενης should mean sprung from Python. To be correct, it should be Pythontogenic: or better from φθιτος Phthitogenic, sprung from decayed things.

considered the line of demarcation between typhus and typhoid fevers to be sufficiently trenchant:—"We shall recognise a practical, clinical difference in the course, duration, symptoms, and secondary lesions of the two morbid processes." He admits also a diversity of causation,—typhus arising more from a depressed state of the system, as the effect of deficient nourishment; typhoid, from the poisonous agencies connected with decomposing matter. If these characteristics be sufficiently definite, the two diseases are not mere varieties of a single species, modified by the influence of climate, diet, occupation, &c.; but two diseases, as distinct as, in the genus *canidæ*, the species canis would be from the species lupus. There is no author who has drawn the line more decidedly than Dr. Jenner; yet, while we admit the force of the leading differentiæ, we think that he has not strengthened his case by the number of minute shades which he has contrasted in tabular columns.

The author's account of the symptoms of typhus is clear and full. Amongst them he has noticed an apposite name applied to the shrunken and shrivelled tongue of typhus by our volatile neighbours, as "la langue perroquet," or parrot's tongue. We observe a remark upon the influence of civilization on typhus, which agrees with our experience—namely, that in the higher and middle classes of society it is fatal by cerebral complication, from the great strain of mind called forth in the battle for the prizes of life; while in the humbler classes it arrives at the same fatal issue by pulmonary complication.

In speaking of the state of the circulation in typhus, Dr. Lyons has noticed more pointedly than we recollect to have seen elsewhere a singular want of harmony among the parts of the arterial circulation in certain cases:—

"We will sometimes find the carotids acting with great violence, while the radial pulse is not sensibly disturbed; in other cases, the temporal vessels will be those acting with excessive force; but the most singular state is that in which, with moderate force and volume of the heart and radial pulse, the abdominal aorta or the iliac arteries, or both, are felt to act with inordinate vigour. There is nothing more striking in the course of fever than the manner in which the several parts of the vascular system seem to act with almost perfect independence of each other."

The author's directions upon the medical treatment of typhus are so comprehensive and important, that we would not do him justice by giving them in a curtailed form. We must make an exception to this, by quoting his remarks upon the use of turpentine in the suffocation of typhoid bronchitis:—

"Turpentine constitutes a remedy of extraordinary efficacy in many cases of typhous bronchitis, when almost at the last gasp. In doses of from 15 to 20 drops, spirit of turpentine is a powerful expectorant; in extreme cases, it may be employed in drachm doses. It may be given in almond mixture, with the addition of a drachm or two of chloric ether to the eight-ounce bottle . . It seems to act as a specific irritant upon the bronchial membrane, inducing cough and expulsive contraction of the semiparalytic circular and longitudinal muscular fibres. . . The effects produced in a few hours in these cases is perfectly astonishing."

In the general treatment of typhus, the author expresses his marked distrust of "the starvation system," as being based on an erroneous pathology; and he quotes from the touching and appropriate eulogy pronounced upon the late Dr. Graves by his eminent colleague, a saying of the former, that when he died, the most honourable memorial which he would wish to have inscribed on his tomb was, "that he had fed fevers." There is little doubt that the opposition to the administration of sustenance had been carried too far, and that since Dr. Graves drew attention to the fact, a salutary reaction has occurred. As a disciple of the Meath Hospital, we should look to the author for kindred views. Accordingly, he enters fully into the regimen of the typhus patient, and even gives a bill of fare for the day, which certainly cannot be accused of stinginess. Thus, instead of maintaining a perpetual current through his patient's primæ viæ according to the received notions, "by physic and slops," to his great discomfort, he gives him for breakfast, tea or coffee and toast; for lunch, a biscuit and a couple of glasses of sound wine; for dinner, good strong beef-tea and toast again, or a little roast or boiled, if the patient desire it; an unexciting supper of arrow-root, at eight o'clock, follows; with a couple more glasses of sound and generous wine, as a nightcap, to close the day. This is certainly an improvement upon the washing and scrubbing system aforesaid, and decidedly much more palatable to the patient himself. The latter, we take it, will not be slow to award, with M. Argan, a well-deserved compliment to his doctor, in which we heartily join:—"Ce qui me plait de Monsieur, mon médecin, c'est que ses parties sont toujours fort civiles."

We had marked some passages in the chapter on typhoid fever, such as the broad line of distinction to be observed in its treatment from that of typhus—the more guarded use of stimulants—the value of quina—and the careful management of perforation of the intestines; but we must desist—

> "Nam mihi commotâ jamdudum mulio virgâ
> Innuit."

We cannot, however, dismiss the present work without observing that it needs no laboured panegyric from us; it is itself its best praise. It embodies the results of great and untiring industry, extensive knowledge, and critical discernment; and is creditable to the school of medicine in which the accomplished author received his education. It would be unfair to the publisher not to acknowledge the way it is brought out. To the zealous student of febrile diseases, we say, go, buy it, and judge for yourself; when you have mastered its contents, you will possess no mean knowledge of fever.

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*A Year Book of Medicine, Surgery, and their allied Sciences, for* 1859. Edited by Dr. Harley, Dr. Handfield Jones, Mr. Hulke, Dr. Graily Hewitt, and Dr. Odling, for the New Sydenham Society. London: 1860. 8vo. pp. 536.

This publication of the New Sydenham Society is little more than a "Catalogue raisonnée," and yet as such is neither accurate nor complete, being replete with errors, typographical and other, and incomplete, inasmuch as it omits all mention of many very important papers published within the period it professes to embrace.

The council of the society have already announced that their year book for 1860 is to be on an improved plan, and to give much more matter. That it may be greatly improved, cannot be doubted; but we can and do doubt very much the advisability of their publishing such a book at all: certainly, in its present form, we think it must be valueless to all the members of the society, except those few who are engaged more in literary pursuits than in the practice of their profession; and even to them it can only serve as an index, directing them to where they may find papers on the subjects they may have in hands, for the abstracts are neither complete enough, nor clear enough, to be themselves depended on. The abstracts from papers of practical interest are often so short and sketchy, as to be more likely to lead their readers astray than otherwise.

This system of republishing abstracts of all the important papers that appear from time to time in the various periodicals is one of very doubtful propriety and prudence; it is most injurious to the journals, interfering with their circulation and legitimate sources of support; it is like killing the goose that lays the golden eggs, and cannot fail ultimately to be injurious to the science and profession of medicine.

These year books, abstracts, and retrospects, engender a habit of desultory and unimproving reading, whereby loose habits of thought are created, or rather thinking is prevented; and as the ploughman idly whistles for want of thought, so the reader of such productions turns page after page, and thinks never. Let the Sydenham Society continue to publish such admirable monographs as they have hitherto done, and they will thus insure their own success, further the progress of our science, and serve the best interests of their subscribers.

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*On Myalgia: its Nature, Causes, and Treatment; being a Treatise on painful and other Affections of the Muscular System, which have been frequently mistaken for Hysterical, Inflammatory, Hepatic, Uterine, Nervous, Spinal, or other Diseases.* By Thomas Inman, M. D., Lecturer on Practice of Medicine, Liverpool Royal Infirmary School of Medicine, &c. London: Churchill. 1860. 8vo, pp. 307.

The author of the work whose title stands at the head of this article is evidently one of that energetic and untiring class of men, who, having taken up a subject in right earnest, appear to be inspired with the determination of working it to the utmost, until at length their mind becomes so thoroughly absorbed in their favourite topic, that, gradually and by insensible shades, a number of affections formerly supposed to be well understood, and to be easily referable to their true positions in the nosology, are one by one constrained to arrange themselves in the favoured category. Old and familiar phases of disease assume a new aspect, and observers of well-known facts arrive at the facile capability of investing said facts with an interest at once novel and startling, because necessarily at variance, in a greater or less degree, with preconceived or established notions regarding their interpretation. The very title of the present work would indicate that the views of the author have undergone some degree of modification within a few years; for, from considering at first that he had to deal with a number of simple cases which could readily be recognised as being purely muscular in their origin, he described them under the title of "Certain Painful Muscular Affections" in 1856; but on finding that the information he had gained was capable of a wider application, and threw much light on the mysterious subject of spinal irritation, he published, in 1858, an extension of the former work, under the title of "Spinal Irritation explained." this latter was reviewed in our 26th volume. "A more ex-

tended experience," he says, "proved that the subject had far wider ramifications than were at first dreamed of." The author has met with a great number of instances in which patients have been under treatment for months for a supposed *uterine* disease, while in reality they have been suffering from pain in the pubic insertion of the rectus abdominis. He has been able to show that a great number of those pains habitually spoken of as *hysterical*, have no real connexion with that disease; and that the very signs relied on were, in reality, proofs that the symptoms were of a purely muscular origin. He was further enabled to throw considerable light upon some of the prominent symptoms of *pleurisy* and *peritonitis*, and to show that many cases called by the latter name were purely myalgic in their nature. This being so [?], it was clear that the subject of muscular pain had as important a bearing upon hysterical, inflammatory, and uterine diseases, as it had upon spinal affections; and to designate his work simply "an explanation of spinal irritation" would imperfectly describe its tendency.

The *naïveté* with which the author describes the difficulty of selecting an adequate title, and how he got over it, tempts us to transcribe the next paragraph. It appears that he "was guided to the choice of the word 'myalgia,'—first, because it had a familiar look about it, as resembling neuralgia; secondly, because it implied no other theory than that the muscles were the seats of pain; thirdly, because it had a more classical sound than the more homely phrase 'muscular pain,' which, by its frequent repetition, would suggest to some facetious patient to dub the doctor by the ignominious title of muscle-man." It may not, perhaps, have occurred to him that, albeit the term is orthographically correct, and profoundly classical as to sound, it is nevertheless suggestive of the notion that although the sundry pains and aches which have a special seat have long since been designated by terms ending in *algia*, these terms are no longer the conventional property of practitioners, but have passed (thanks to the diffusion of knowledge) into colloquial use among patients; and that while he has ingeniously succeeded in anticipating the Mahometan allusion of the supposed facetious patient, some equally thoughtless *bel-esprit* may recognise in the newly coined term, a simple appropriation on the part of the doctor, who would fain designate the fashionable ailment by the name of *my* algia to distinguish it from the algias of other men.

In order that our readers may be enabled to form an idea of the general drift of the present work, it may be stated that the author endeavours to show, by a lengthened series of ob-

servations, illustrated throughout by the details of numerous cases—1. That the symptoms attributable to "spinal irritation" have nothing to do with the spinal cord, or the nerves arising from it. 2. That the majority, if not the whole of them, are due essentially to a cause similar to that which produces the spinal tenderness. 3. That the spinal tenderness results from over-straining of the fibrous origins of the muscles attached to the spinous processes. 4. That the spinal tenderness is analogous to that experienced at the origin and insertion of muscles in other parts of the body. 5. That the weaker the individual is, the greater is the tendency to fibrous pain. 6. That the most common causes of the pain and tenderness in any part of the muscles, are constitutional or acquired debility, coupled with too great an amount of work. Therefore, as soon as the diagnosis of muscular pain is made, the next inquiry should be into the causes which have produced the debility, supposing that no extra exertion has been made. 7. That debility increases equally the irritability of the muscular and the nervous systems. 8. That before hysteria can manifest its presence, there must be debility from some cause or other. 9. That that debility may show itself in the muscular or nervous system, or both. 10. That debility affects the nervous system as a whole or in sections—that is, mental, sensitive, motor, organic. 11. That functional affections in any one or more of these parts have long been recognised as emanating from deficient vital power. 12. That anything which deteriorates the vital power has a direct tendency to aggravate the complaints referred to. 13. That muscular and nervous irritability are subject to the same laws, and that the remarks applicable to the one are *mutatis mutandis*, applicable to the other. 14. That many symptoms hitherto supposed to be due to hysteria, spinal irritation, or nervous irritability, to pleurisy, peritonitis, uterine, or stomachic disease, are essentially muscular in their origin, and are produced by fatigue in enfeebled constitutions, and that the general opinions respecting the diagnosis of hysterical symptoms require a complete remodelling. 15. That the link connecting hysteria with spinal disorders is constitutional or acquired debility. 16. That, as regards curious mental phenomena, excess of sensibility in the nerves of common or special sensation, or a propensity to spasmodic actions, and to irregular organic phenomena, there is no essential distinction; they are simply different facets of the same die. 17. That the essential distinction between genuine hysterical and muscular affections is, that a large amount of bodily rest is necessary for the cure of the

latter, while it is not so absolutely requisite for the former. That the former are difficult, the latter comparatively easy of cure. 18. That for the future, it will be necessary to discriminate between pain arising from muscular fatigue, cramp, or fibrous stretching, genuine neuralgia, and hysteria; and that there will be neither precision in diction nor clear idea of treatment until the distinction is made.

From the foregoing recapitulation of the author's views regarding the nature of myalgic suffering, the reader will find but little difficulty in deducing the principles of treatment suitable to the various cases in which muscular pain constitutes a prominent or essential feature. The importance of observing such rules as relate to the economising of a patient's strength, and of promoting its increase by suitable measures; the value of change of air or of occupation, as well as the necessity of directing a generous and nutritious diet, with such tonics as the particular cases may require; all this is copiously treated of towards the latter part of the volume; indeed, in this, as in every portion into which the work is divided, the minuteness with which the most ordinary and commonplace particulars are dwelt upon is, in our opinion, better adapted for the listless perusal of some *malade imaginaire* than calculated to meet the healthy requirements of the busy and hurried practitioner, who would be content with a bold and broad outline of facts and principles, trusting to his own general knowledge and experience for filling in the details. Before we conclude, we must extract a passage from the chapter on the *diagnosis* of muscular pains, nor is it the only place in which a case adduced "in illustration" appears to us to have been rather inapposite, and capable of an interpretation somewhat different from that which the author would suggest:—

"When the pain is a fixed one, it almost invariably occupies a spot which is more or less tendinous, or where the muscles are attached to the bones; and this should at once enable us to distinguish it from neuralgia. There is, however, one exception to this point of diagnosis, to which I must refer. A woman cannot be expected to point to the pubic insertion of the rectus,—she always points to the hypogastric region instead; but, as far as my experience goes, the practitioner will have no difficulty in gaining the information he requires by indicating in his own person [!] the precise seat of pain. I find, that many women suffer pain at the origin of the lesser pectoral, which they do not tell of, lest it should necessitate an examination of the mammæ. One patient, who described all her other symptoms fairly, omitted this; but when I questioned her, she answered that her breasts were often so acutely tender that she could not bear to touch them. She was a very stout,

florid-looking girl, with abundance of energy and spirit. The cause of the debility and muscular exertion was, in her case, excessive vomiting. I suspected pregnancy, and heard that she subsequently aborted."

Leaving our readers to make such comments upon this case as their several notions thereof may suggest, we proceed, in conclusion, to notice what we conceive to be by far the best part of the book, namely, the lithographic illustrations. The delicate finish of their execution is in the highest degree creditable to the artistic skill of Mr. Bagg; and with the exception of the *pose* of the first figure, which is not quite conformable to correct rule, the most critical can find no fault in the accuracy of the designs which that artist has so graphically pourtrayed. What we are disposed to object to is, the indelicate misemployment of the drapery held in the right hand of the young lady who faces the title page; were this figure *completely* nude, the "model" might plead the very natural excuse of "nothing to wear;" but as this is not the case, the artist would have consulted grace, as well as delicacy, had he contrived a more judicious disposal of the drapery, without encroaching upon even the inferior boundary of the myalgic landmarks.

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*Course of Lectures on the Physiology and Pathology of the Central Nervous System*, delivered at the Royal College of Surgeons of England in May, 1858. By C. E. Brown-Séquard, M.D., &c. London: Williams and Norgate, 1860. 8vo, pp. 118.

This is a reprint of the lectures delivered by Brown-Séquard, at the College of Surgeons, London, in 1858, which contain, the author says, the results of the work of almost all his life, since he began to study medicine. With the modesty of true genius he invites criticism, acknowleding that as the questions discussed are as difficult as they are important, and as many of them are quite new, so that he could not have the benefit of the views of other authors to guide him in their examination, he may have come to erroneous conclusions on many points. It is in the power, however, he says, of most medical practitioners to prove or disprove his views; for out of the millions of patients yearly treated by those who may peruse the book, there are indeed many thousands whose cases may throw light, and often a decisive light on the questions discussed.

And as most of these questions are of the greatest importance, in both a practical and a scientific point of view, he expresses a hope, in which we cordially unite, that those who will peruse the book will not let the cases pass unrecorded that may be the means of settling what is yet undecided in the matters considered.

But the criticism invited is the criticism of facts, the daily observations of patient clinical observers: if these confirm the doctrines here put forth, they will establish them on an unassailable foundation; but if the facts derived from the more extended sphere of observation require the theories to be abandoned or modified, we are confident that none will be more ready to do so than the author himself. We propose here simply to state the doctrines deduced by Brown-Séquard from his researches, with a view to direct attention to them, and to recommend all interested in the advancement of medical science, and all anxious to clearly understand the functions of the spinal cord, and the diseases to which it is liable, to study the lectures for themselves, which are now published for the first time in a separate form. The lectures as delivered were only six in number, but they are now extended to twelve, being swelled out by the addition of a great variety of illustrative cases and pathological observations; an appendix is added, containing additional facts, and an examination of the objections that might be raised to the doctrines propounded, and of the principles of treatment that might be deduced from the theories established.

The first lecture opens with a statement of the mode of investigation adopted; it is shown that while no means that science may furnish should be neglected, the results of experimentation upon living animals, and observation of pathological cases, when taken together and compared, are most worthy of reliance. We have then some observations on the functions of the anterior and posterior roots of the spinal nerves, and some new arguments adduced as to the truth of Sir Charles Bell's statement that these preside respectively over motion and sensation from the mode of decussation of their conductors. The "Recurrent Sensibility" of Majendie is traced to its true cause. Majendie discovered, as is well known, that irritation of the anterior roots caused pain, but in a less degree than the posterior roots. To explain this, it was suggested that a branch of nerve leaves the posterior root where it unites with the anterior, and returns in the anterior to join the spinal cord, and such a nerve has been actually pourtrayed in some of our elementary works on physiology. The author, however, shows

that the signs of pain observed by Majendie really arise from impressions made on the peripheral extremities of the fibres of the posterior roots that are distributed to the muscles that are thrown into contraction by the irritation of the anterior roots. He refers to the experiments of Matteucci, as showing that each time a muscle contracts, some change in its galvanic condition is effected. This change affects the nerves of sensation, and hence the signs of pain discovered by Majendie. He shows, moreover, by an experiment of his own, that it is the tendency to contract that causes the pain, rather than the actual shortening of the muscular fibre, and that the greater the resistance to the shortening of the muscle, the greater the pain, and in this way he accounts for the pain of various muscular affections.

"If we compare these results with the following pathological facts, we find that the phenomena are much alike in the two series of facts that we compare, and, therefore, that they seem to depend on the same causes. I suppose a case of painful contracture of the anterior muscles of the thigh; the pain is increased very much every time the contracted muscles are elongated, *i. e.*, when the resistance to the contraction is augmented; on the contrary, it diminishes when the resistance to the contraction is rendered less than it was, and, at last, *it disappears entirely, or almost entirely, when the resistance is completely, or almost completely, destroyed*, after tenotomy. Surgeons, till our researches, had not been able to explain this apparently strange cessation of pain; now it seems quite simple to understand that such should be the case.

"In cases of *fissura in ano*, it is very well known that the pain due to the spasm of the sphincter is increased when there is a resistance to the contraction, and that the greater the elongation of the muscular fibres, the greater also the resistance to their contraction and the degree of pain. At last, when the muscular fibres can contract freely, and almost without resistance. the pain disappears, as is the case after the operation of Boyer (the section of the sphincter). Of course the pain depending upon the fissure persists, but that due to the muscular spasm disappears. In this case, also, surgeons could not explain the cessation of pain. We find here, as in the preceding case, that the excitation of the nerves due to the muscular contraction augments, decreases, and disappears in the same circumstances, concerning the degree or the absence of resistance, which produce analogous phenomena in the experiments above mentioned.

"In cases of neuralgia, when the sensibility of nerves in muscles is increased, there is pain produced or increased every time the muscles contract.

"The contractions of the uterus, which are the more painful the more there is resistance opposed to them, cause also pain in the same way as the spasm of the anus or the contraction of the mus-

cles of the thigh. The relation between the degree of contraction of the uterus and that of pain is so evident, that the word 'pains' is employed for that of 'contraction.'

"Every muscular contraction seems to generate a galvanic excitation of the sensitive nerves in the neighbourhood of the muscular fibres; and, the degree of excitation being in proportion to the degree of energy of the contraction, we have, in this way, an excellent means of judging of the state of the muscles. When the contraction acquires the degree which it has in cramps, then it causes pain, which is also in proportion to the energy of the spasm. If we succeed when we have a cramp in making it cease by elongating the contracted muscle, we find that the pain often increases at first, and disappears only when the contraction has ceased. I regret not to be able to bring forward all the reasons which have led me to admit the views I have just proposed, but I must keep within my programme.

"If, now, we examine what takes place in the apparently paradoxical experiment of Magendie, we find that nothing is more easily explained. When the anterior roots of a spinal nerve are excited, a cramp, is produced in the muscles in which the nerve-fibres of the roots are distributed, and the pain which belongs to a *cramp* is generated. I think this pain is due, as I have said for cramps, to a galvanic excitation of the sensitive nerve-fibres existing in the muscles which contract; but whether this theory be true or not is, in a measure, indifferent as regards the general cause of pain in the experiment of Magendie. In fact, there is then the same cause which exists in a cramp; and this cannot fail to be so inasmuch as a real cramp is generated by the irritation of the anterior roots. So, then, we can conclude: 1st, that the *recurrent* sensation is only in appearance recurrent; 2d, that the anterior roots of the spinal nerves cause pain when they are irritated, because they produce a cramp: 3d, that, consequently, *there is no sensibility of any kind in the anterior roots, and that it is because they are motor, and not because they are sensitive, that they cause pain when they are irritated.*

"Therefore, the objection which has been urged against the views of Sir Charles Bell, and which was founded upon the fact that the anterior roots cause pain when irritated, is unfounded."

We have next considered the statement of J. W. Arnold, that the nerve-fibres that convey the knowledge to the sensorium of the state of the muscles are contained in the anterior roots; and while it is admitted as very probable from the facts referred to by Arnold, that some such fibres may pass up through the anterior roots, yet it is shown that it is not thus only that the brain derives a knowledge of the state of the muscles, and that certainly all fibres that convey a sense of pain and a sense of touch pass through the posterior roots.

But while the truth of Sir Charles Bell's discoveries as to

the functions of the roots of the spinal nerves is thus demonstrated, it is shown that his ideas as to the functions of the columns of the spinal cord were incorrect. It is well known by a generalisation, perhaps natural, but very hasty, Sir Charles inferred that the posterior columns of the spinal cord were the continuation of the posterior roots of the spinal nerves, and presided, like them, over sensation, and the anterior columns over voluntary motion. This theory he supported by a very imperfect experiment; it was opposed, however, by many eminent physiologists; and at length Sir Charles, finding, as he says himself, that the theory barred his progress, renounced it in a paper read before the Royal Society in April, 1835, adopting instead the opinion that the lateral columns must be the parts transmitting sensitive impressions.

Notwithstanding, or perhaps unaware, that Sir Charles had abandoned his first theory on this subject, Longet, a French physiologist advocated it so ardently in 1842, that it was soon very generally received, and even taught, in some of our class-books. Todd and Bowman, however, assailed it in their valuable work on physiological anatomy; and Todd gave it the *coup de grace* in his most able and comprehensive article on the physiology of the nervous system in the Cyclopœdia of Anatomy.

Brown-Séquard devotes his second lecture to the consideration of the functions of the posterior columns. He refers to the anatomical researches of Todd, Lockhart Clarke, and others, as showing that the posterior roots are not connected with the posterior columns of the spinal cord. He criticises the statements of Longet, showing that they are so self-contradictory as to be untenable, and that he overlooked important elementary principles in his experiments, and then proceeds to develop his own views, and explain the experiments on which they are founded. For these we must refer our readers to the work itself, as we cannot do more than briefly summarize them here. First, it is shown that division of the posterior columns, instead of depriving all parts of the body behind the section of sensation is followed by marked hyperæsthesia, the rationale of which is to be explained hereafter. 2ndly, it is found that division of all the cord, except the posterior columns, destroys sensibility. 3dly, Dr. Brown-Séquard shows that the lateral columns do not convey sensibility (the theory adopted by Sir C. Bell, when he found that the posterior columns did not perform this duty). He shows this by an experiment in which he divided the lateral columns; when sensation, instead of being lost, was increased, as in the case of the posterior. 4thly, he shows that the grey matter is the principal conductor of sensations in the

spinal cord, the anterior columns assisting in a slight degree. 5thly, that it is the central part of the grey matter that conducts the impressions—that the anterior horns may assist in this, but that the posterior take no part in it. 6thly, that the grey matter conveys sensations by means of the connexion of the cells with one another and the white fibres surrounding the grey substance. 7th, that the conductors of impressions either enter the central grey matter directly, or pass to it through the posterior, or posterior part of the lateral columns, or through the posterior horns of the grey matter, and in these run both upwards and downwards for a short distance before joining the grey central matter. 8thly, that the restiform bodies which are the continuations of the posterior columns do not give transmission to any of the conductors of sensitive impressions of either the various parts of the trunk and limbs or of the head; and that therefore the cerebellum, with which the restiform bodies are connected, does not receive from them any such conductors.

The third lecture contains an account of the most interesting and novel contribution, (as we believe) that Brown-Sequard has made to physiological science, viz., the fact that the conductors of sensation decussate with one another in the spinal cord—that the decussation is complete in mammals, but not so much so in birds and reptiles—and that it takes place at a short distance from the points of insertion of the posterior roots. These inferences are drawn from a series of experiments which seem to establish them completely:—

"1st. The spinal cord of a mammal is laid bare at the level of the two or three last dorsal vertebræ, and a lateral half of this organ (including the posterior, the lateral, and the anterior columns, and all the gray matter on one side) is divided transversely. The animal is left at rest for a little while, and then it is ascertained that sensibility seems to be much increased in the posterior limb on the side of the section, while it seems to be lost, or extremely diminished, in the posterior limb on the opposite side. There seems to be, therefore, *hyperæsthesia* behind and *on the side* of a transversal section of a complete lateral half of the spinal cord; while, on the contrary, there seems to be *anæsthesia* behind the section, and *on the opposite side.*

"This experiment is one of the two made by Galen; but he seems not to have looked at all at the condition of sensibility, and he simply states that there is a paralysis on the side of the section, and no paralysis on the opposite side.

"Sir Astley Cooper, under the suggestion of Dr. Yelloly, has made a similar experiment, except that the section was higher; the

state of sensibility is not mentioned, and, as regards movements, there was paralysis on the side operated upon.

"Schœps, Van Deen, and Stilling have observed that sensibility is not lost in the limb or limbs behind, and on the side of the section of a lateral half of the spinal cord; but they have not remarked the most important fact, that on the opposite side there is anæsthesia. They also do not mention this curious result of this experiment, the existence of hyperæsthesia on the side of the injury.

"Fodéra was very near discovering that there is a decussation of the sensitive fibres in the spinal cord. He says he has found in some cases, that a section of one of the posterior columns caused a diminution of sensibility in the opposite side of the body; but he states that in other cases he has seen the reverse. He also has sometimes remarked that the section of one of the posterior columns causes hyperæsthesia in the same side, and that a section of these two columns produces hyperæsthesia in the two sides, but he declares, also, that he has seen the reverse.

"Two explanations for some of the results of a section of a lateral half of the spinal cord may be proposed, as regards sensibility. Either it may be imagined, as it has been by several German physiologists, that the gray matter has the power of transmitting sensitive impressions in such a manner that one lateral half of this substance is sufficient for the two sides of the body, or that the conductors of sensitive impressions decussate in the spinal cord, so that those which come from the left side of the body pass into the right side of the spinal cord, and *vice versâ*. The hypothesis of the Germans may explain the fact that sensibility persists on the side of the section, but it is proved to be absolutely inadmissible by the fact that there is anæsthesia on the opposite side. We will see that the other experiments we have to mention are also in opposition to the view of the Germans (Stilling, Schiff, and others). On the contrary, all the facts concur to prove the existence of a decussation.

"2d. If, after having made a first section of a lateral half of the spinal cord in the dorsal region, on the right side, for instance, and after having ascertained that the *right* posterior limb is hyperæsthetic, or at least extremely sensitive, we divide the *left* lateral half of the spinal cord in the cervical region, we find then that the *right* posterior limb loses entirely, or almost entirely, its sensibility. This experiment shows clearly that the sensitive impressions coming from the *right* posterior limb, after the first section, passed across the spinal cord from the right into the left side, along which they were transmitted to the encephalon.

"3d. To obtain a very striking result from the experiment which consists in only one section of a lateral half of the spinal cord, it is better to make it after the posterior columns have been divided. We know that after this division there is hyperæsthesia in the parts of the body which are behind the section; if, after having ascertained this fact, the section of a lateral half is completed where the

posterior columns have been divided, we find that the hyperæsthesia seems to increase on the side of the second operation, while, on the opposite side, not only the hyperæsthesia, but sensibility entirely disappears.

"4th. There is another mode of proving that the conductors of the sensitive impressions decussate in the spinal cord. In several points of view this mode of demonstration is superior to the preceding. It consists in a longitudinal section of the spinal cord, an experiment already made by Galen, but the results of which, as regards sensibility, have been overlooked by him.

"The spinal cord is laid bare in the whole lumbar region, and a careful division of the entire extent of the part of the organ giving origin to the nerves of the posterior limbs, is made so as to separate the two lateral halves of the organ, one from the other. If this experiment could be executed perfectly well, nothing would be divided in the cord except the commissures, which unite the right side with the left side of the cord, and all the longitudinal elements of this nervous centre would be left uninjured; but it is impossible not to cut more or less on either side. However, when the operation has succeeded well—*i. e.*, when the two separated halves have been very little injured, a striking result is obtained. The voluntary movements still exist in the posterior limbs (though diminished on account of the injury to the muscles of the lumbar region), but *sensibility is entirely lost* in them. To those who know that injuries to the spinal cord, which cause a diminution of sensibility, always produce a greater diminution of voluntary movements, this fact will not be explained by the supposition that some injury has, then, been made to the two halves of the cord, and that it is in consequence of this supposed injury that the loss of sensibility is due. At least it will, I think, be easily admitted that if the two lateral halves of the cord had been injured enough to produce a complete and lasting anæsthesia, there would be a notable degree of paralysis of voluntary movements. We repeat that such is not the case: the animal has the use of his two limbs; he moves about pretty freely, as Galen had already said. The loss of sensibility, therefore, must depend on the section of the commissures of the spinal cord, or, in other words, on elements of this organ which cross each other in the median line, or, rather, the median plane.

"If now we compare the results of this experiment with those of a transversal section of a lateral half of the spinal cord, we find that they agree perfectly in showing that the conductors of the sensitive impressions decussate in this organ. It is useless to stop to show that the longitudinal separation of the lumbar enlargement of the spinal cord would not produce anæsthesia, if the German physiologists were right in admitting that the gray matter has the power of transmission in every direction. It would be useless, also, to insist upon the disagreement between the results of a longitudinal section of the spinal cord, and the views of the physiologists who admit that the posterior columns are composed of sensi-

tive fibres, coming from the posterior roots, and going up to the encephalon. These columns are left almost entire and uninjured, and nevertheless, sensibility is lost.

"5th. Another experiment, which is a combination of two of the preceding, gives a still better proof of the decussation of the conductors of sensitive impressions in the spinal cord. A longitudinal section is made on the cervico-brachial enlargement of the spinal cord, so as to separate it in two lateral halves. I ascertain then that sensibility is lost in the two anterior limbs, while it remains, and even seems to be increased, in the two posterior limbs. Of course, if the loss of sensibility in the two anterior limbs depended upon an injury to the two sides of the cord, and not upon a section of the decussating conductors of sensitive impressions, there would be a loss of sensibility, or at least, a diminution of it in the posterior limbs. The admission of a decussation explains the two facts: loss of sensibility in one set of limbs, and conservation in the other set. If we divide transversely, in the same animal, the right lateral half of the spinal cord, we find then that the posterior limb on the same side becomes more evidently hyperæsthetic than before, and that the left posterior limb loses its sensibility. The transmission for this last limb therefore took place by the right half of the cord, while that for the right posterior limb continues to take place by the left half of the cord."

Lecture IV. opens with asserting that the senses of touch, pain, temperature, and of muscular contraction, have each of them separate conductors, and proceeds to show that these conductors do not proceed to the brain along the posterior columns, notwithstanding the assertion of Moritz Schiff that the conductors of tactile impressions do pass along these columns; and it is also shown that all these conductors decussate, as already described. The lecturer then asks how is it that sensibility is not lost, and is only more or less diminished, although the spinal cord is deeply affected? In reply, he shows that the conductors of sensation are diffused throughout the whole substance of the cord except the posterior columns, a view which explains the so frequent persistence of sensibility in cases of disease of the spinal cord:—

"If, for instance, we imagine that there are a thousand conducting elements coming from a small part of the right side of the body, in the left half of the spinal cord, they are scattered in all the parts of the conducting zone of this half, so that to divide them all, a section must divide the whole of this zone. In other words, we can say, that *every small portion of the conducting zone in a lateral half of the spinal cord contains conductors of sensitive impressions coming from all the points of the body on the opposite side, which are behind the place of this small portion.* We can say, also, that *the sensitive*

*impressions made on any point of the lateral half of the body are transmitted to the sensorium by conducting elements, distributed in all the parts of the lateral half of the spinal cord on the opposite side.*

"This view, which explains the so frequent persistence of sensibility in cases of disease of the spinal cord, is entirely different from that of Stilling and others who admit that a part of gray matter in one half of the cord is sufficient for the transmission of sensitive impressions from both sides of the body."

The author now passes to the consideration of the transmission of the orders of the will to the muscles through the spinal cord. He finds it is very much more difficult to determine what are the parts of this organ employed in voluntary movements than to find out what are those through which the sensitive impressions are transmitted. The results on this point at which he arrives are, that in the dorsal region all the various parts of the spinal cord, except the posterior columns, are employed in the conveyance of the orders of the will to muscles—that in this region the *anterior* columns and the grey matter are the most important; and of the grey matter that in the anterior half of the cord, in the conveyance of the orders of the will; but near the medulla oblongata he finds the *lateral* columns and the grey matter between them the most important. In some animals there are reasons to believe a decussation of the volitional fibres takes place to some extent in the spinal cord; but pathological facts do not bear this out as occurring in man, in whom the crossing of the anterior pyramids in the medulla oblongata is the only decussation that takes place. The lecture concludes by showing that the results of experiments afford no support to the assertion of Bellingeri and Valentin that the motor fibres that go to the extensor muscles pass through the posterior columns, while those that go to the flexor muscles go through the anterior columns.

Lectures v., vi., vii., and viii. are taken up with the examination of pathological cases, in which the symptoms have been carefully observed during life, and the morbid appearances faithfully recorded. Some of these had been observed by the author himself; but the greater part have been published by various physicians in all parts of the world, a circumstance adding not a little to their value. They fully bear out the principles laid down in the previous lectures, and we would earnestly recommend their careful study to all our readers. The first group of cases shows clearly that the posterior columns may be deeply altered; and yet, sensation in the limbs be perfect or even exaggerated, proving that they are not the sensory

tracts, and the same is proved of the restiform bodies. The next group shows that the grey substance is the principal channel for the transmission of sensitive impressions. We have then two series of cases proving the decussation of the conductors of sensation. The first series of these are summed up as follows:—

"Setting aside, in this lecture, what relates to temperature, we state that a transversal section of a lateral half of the spinal cord causes a loss of voluntary movement in the corresponding side of the body, and a loss of sensibility in the opposite side, and if, now, we look at the cases we have related to see how they agree with this general result, we find the following points:—

| Cases. | Side of the injury. | Side of the paralysis. | Side of the anæsthesia. | Side of conservation of sensibility, or of hyperæsthesia. |
|---|---|---|---|---|
| 29 | *right* | *right* | *left* | *right* |
| 30 | *left* | *left* | *right* | *left* |
| 31 | *right* | *right* | *left* | *right* |
| 32 | *left* (probably) | *left* | *right* | *left* |
| 34 | *right* (ib.) | *right* | *left* | *right* |
| 35 | *left* (chiefly) | *left* (chiefly) | *right* | *left* |

"If we add to these cases those in which we have no other reason but the analogy of symptoms with those observed in animals, such as Cases 33 and 37, we have two more instances of this curious but simple morbid manifestation—*i. e.*, loss of movement in one side, loss of sensibility in the other. In animals, as I have often said and shown, the paralysed parts are in a state of hyperæsthesia; in the cases I have related, hyperæsthesia has been noted in Cases 32, 33, 35, and 37. It was so great that the least touch produced pain in Cases 32 and 35. It would have been found in the other cases, had the physicians who attended the patients looked for it."

The eighth lecture reviews the conclusions to be drawn from the cases detailed in the preceding lectures. It is shown that these cases prove that the posterior columns are not the channels for the transmission of the orders of the will to the voluntary muscles; and that paralysis of voluntary motion, is not a symptom of section or local destruction of these columns. It is also shown that the clinical facts are in opposition to the theory of Bellingeri, that the posterior columns supply the extensor muscles. Spasm of the flexor muscles of the lower limbs—spasm so powerful, that the thighs come almost in contact with the abdomen, while the heels are drawn up so as to touch the back parts of the thighs—is a symptom that seems to belong exclusively to disease of the spinal cord, but does not belong exclusively to disease of either the anterior or posterior columns, as regarded by Bellingeri's theory.

On reviewing all that is known of alteration of the posterior columns, it is found that, when limited to a small extent, it does not affect voluntary movements; but when the alteration extends to a few inches in either the cervical or dorso-lumbar regions, these movements are always more or less impaired; that when the cervical region alone is affected, the upper limbs are paralysed, and not the lower, except from other causes. Destruction of the whole length of the posterior columns is said to be found with complete paralysis; but, on examining the cases, Brown-Séquard concludes that the paralysis was due to other parts of the cord being affected. Alterations of limited portions of the posterior columns do not interfere with reflex movements; but, if extensive, prevent these movements and so enfeeble voluntary ones. If the entire length and thickness of these columns in the lumbar region be affected, the patient is unable to stand or walk because of the loss of reflex actions, but when lying in bed is able to move the limbs pretty freely.

The *lateral* columns of the spinal cord, near their decussation, are the channels of volition, with, perhaps, some fibres in the grey substance; destruction of them here causes paralysis, while that of the *anterior* columns at this point does not; but alteration of these anterior ones, lower down, is always followed by loss of voluntary motion, and this sympton exists also in alterations of the grey matter. Paralysis of voluntary motion alone, then, cannot be of service in the diagnosis of the place altered in the spinal cord; but the various modifications in the degree, in the extent of place, and in the kind of paralysis, and the co-existence of this symptom with others, &c., are able to guide more or less surely in the diagnosis of the seat, and also of the nature of the alteration in the spinal cord.

The lecturer next passes to the consideration of anæsthesia as a symptom of disease of the spinal cord. He finds that it is not connected with disease of the posterior columns, but rather with that of the grey substance. There are, he says, many pathological cases which can only be explained on the hypothesis, that there are different conducting fibres for the several kinds of sensation, as—1. Touch; 2. Tickling; 3. Pain; 4. Heat; 5. Cold; and that these enter different parts of the spinal cord. He further shows that injury confined to any of the white columns does not cause loss of sensation; but that if the lateral half of the cord be affected, there is anæsthesia of the opposite side of the body. The lecture terminates with the following enunciation of the groups of symptoms existing in the cases of disease of the spinal cord

according to the place and extent of the alteration. He has, however, omitted purposely the symptoms concerning the movements of the heart, respiration, the state of the sphincters, animal heat, and nutrition:—

"1st. *Deep alteration of the posterior columns in all their length.*—Increased sensibility in the trunk and limbs for impressions of touch, or due to pricking, pinching, and galvanic excitations, and for changes of temperature (cold and heat). Loss, or a very great diminution, of reflex movements. All kinds of voluntary movements possible, and more or less easily executed when the patient is in bed. Walking and standing very difficult.

"2d. *Deep alteration of the posterior columns in the extent of the cervico-branchial swelling.*—Increased sensibility in the four limbs, and in the trunk, for all kinds of impressions. Diminution of reflex actions in the upper limbs, and increased reflex actions in the lower limbs. Some difficulty in the direction of the movements of the upper limbs, without the help of the sight. Standing and walking possible without any great difficulty.

"3d. *Deep alteration of the posterior columns in the extent of the dorso-lumbar swelling.*—Increased sensibility in the lower limbs, and normal sensibility in the upper ones. Diminution or loss of reflex actions in the lower limbs. Movements of lower limbs possible, and even easy, when the patient is in bed; but walking and standing very difficult.

"4th. *Deep alteration of a very limited part of the posterior columns.*—Increased sensibility, and increased reflex action, in all parts receiving their nerves from the spinal cord below the alteration. Voluntary movements possible, and even easy, everywhere. The place of the alteration may be detected by diminution of reflex actions in the zone round the body receiving nerves from the level of the part altered in the posterior columns.

"5th. *Alteration of the posterior columns and posterior roots of the spinal nerves.*—Instead of hyperæsthesia, as in the preceding cases, diminution or loss of all kinds of sensibility, in places receiving the spinal nerves, which are the continuation of the altered roots. Voluntary movements still possible, in bed, and while the patient looks at his limbs, but walking and standing almost impossible. Reflex action *completely* lost in all the anæsthetic parts. If the alterations are in the upper parts of the spinal cord, the other parts being healthy, then voluntary movements in the lower limbs, and even walking or standing, are possible, and may be easy, and these limbs have an increased sensibility and increased reflex actions.

"6th. *Alteration of the posterior columns and of the gray matter in all their length.*—There is no difference between this case and the preceding, except that here there is a real paralysis of voluntary movements, which is complete if the alteration extends to the anterior gray cornua. Greater frequency of formication and of other sensations referred to the periphery.

"7th. *Alteration of the posterior columns and gray matter in any limited part of the spinal cord.*—Very nearly complete loss of sensibility. Degrees of paralysis of voluntary movements varying with the place occupied by the alteration in the length of the spinal cord. Reflex actions increased in parts receiving their nerves from the portions of the cord below the seat of the alteration.

"8th. *Alteration limited to the gray matter.*—The same symptoms as in the preceding cases, except that at first there is a greater degree of anæsthesia than of paralysis, if the alteration begins in the very centre of the cord. Formication and other sensations referred to the periphery, in cases of inflammation.

"9th. *Alteration of the anterior columns in the upper part of the cervical region.*—No paralysis, no anæsthesia, very slight hyperæsthesia, various sensations (particular pain) referred to several parts of the body.

"10th. *Alteration of the lateral columns in the upper part of the cervical region.*—Paralysis of voluntary movements in the four limbs and the trunk. Increased sensibility and greatly increased reflex actions in the paralyzed parts.

"11th. *Alteration of the anterior columns in any part of their length, except the neighbourhood of the medulla oblongata.*—More or less complete paralysis of voluntary movements in all the parts receiving their nerves from or below the parts of the cord where the alteration exists. Slight hyperæsthesia. Reflex actions very much diminished in the parts which receive their nerves from the altered portion of the cord, and increased below these parts.

"12th. *Alteration of the lateral columns in any part of their length, except the neighbourhood of the medulla oblongata.*—Incomplete paralysis of movements. Hyperæsthesia. Diminution of reflex actions less than in the preceding case.

"13th. *Alteration of the anterior half of the spinal cord, including the anterior columns, a good part of the gray matter, and a part of the lateral columns.*—Voluntary movements completely paralysed. Sensibility very much diminished. For reflex actions, as in 11th.

"14th. *Alteration of the various parts of the spinal cord, except the posterior columns.*—Loss of voluntary movements and of all kinds of sensibility. Reflex actions increased or diminished in certain parts of the body, according to the place of the alteration in the length of the spinal cord.

"I know many cases in which one of the two last kinds of alteration has existed. In another lecture, I will relate three of them, which have been recorded by Mr. Cæsar Hawkins, by Dr. John W. Ogle, and by Dr. T. Inman."

The remaining lectures are "On the sympathetic Nerve;" "On the Influence of the Nervous System on Secretion and Nutrition;" "On Epilepsy;" "On the Medulla Oblongata the Pons varolii, and some parts of the Spinal Cord in their relations with the Respiratory Movements," &c. We cannot

afford room to enter on these subjects, especially as we have very recently discussed them all fully in several reviews. We must, however, before closing, explain the hyperæsthesia so constantly found to occur after division or destruction of the posterior or lateral columns of the spinal cord. The author traces this to paralysis of the vaso-motor nerves. These nerves he believes to arise from the spinal cord, and to be distributed to the vessels on the same side of the body as the portions of the cord they arise from. Division of these portions of the cord paralyses the vaso-motor nerves arising from them, consequently the muscular coats of the small arteries of the same side are also paralysed, the vessels become dilated and there is an increased afflux of blood to the part. As the sensitive nerves of this side cross to the opposite side of the spinal cord on entering it, they are not paralysed by the injury done to the cord, and in consequence of their peripheral extremities having an increased supply of blood a state of hyperæsthesia is induced. Thus we have as a result of division of a lateral half of the spinal cord, loss of voluntary motion on the same side, hyperæsthesia of that side, and loss of sensation with retention of voluntary motion in the limbs of the opposite side.

In taking leave of the subject, we cannot but regret that our author has not adopted fully the theory of the action of the spinal cord propounded by Dr. Todd in that most complete and admirable article of his on the physiology of the nervous system in the "Cyclopædia of Anatomy and Physiology."

We have recently shown in our review of Van der Kolk that the result of all our best microscopical examinations is to confirm and illustrate Todd's theory. We now say the same of Brown-Séquard's experiments and researches, which, while they fully confirm, add but little to what Todd has done; we might almost say nothing, except the fact of the decussation of the conductors of sensation on their entrance into the cord. Some points that are obscure in Séquard's cases might be cleared by adopting the theory that all the spinal nerves terminate in the grey substance of the spinal cord, and that the white columns, instead of being continuations of the nerves through the cord to the brain are commissural fibres connecting the segments of the cerebro-spinal centres with one another. It will be further observed that, though Brown-Séquard devotes much of his lectures to showing what the posterior columns of the cord do *not* do, he nowhere attempts to show, with any precision, what their true nature is, or what functions they *do* perform. Dr. Todd's views on this point are so explicit, and so fully ac-

cord with both the experiments and the clinical observations of Brown-Séquard, that we think it advisable, in order to complete all that we have said, to quote his remarks at length:—

"I have long been strongly impressed with the opinion that the office of the posterior columns of the spinal cord is very different from any yet assigned to them. They may be in part commissural between the several segments of the cord, serving to unite them and harmonize them in their various actions, and in part subservient to the function of the cerebellum in regulating and co-ordinating the movements necessary for perfect locomotion.

"This view is suggested by a comparison of the spinal cord with the brain, and by the anatomical connexions of the posterior columns.

"The brain is an organ composed of various segments, which are connected with each other by longitudinal commissures. The cord is obviously divisible into a number of ganglia, each forming a centre of innervation to its proper segment of the body. These portions must be connected by similar longitudinal commissures to those which confessedly exist in the brain. If we admit such fibres to be necessary to ensure harmony of action between the several segments of the encephalon, there are as good grounds for supposing their existence in the cord as special connecting fibres between its various ganglia to secure consentaneousness of action between them.

"The attribute of locomotive power rests upon the connexion of the posterior columns with the cerebellum, and the probable influence of that organ over the function of locomotion and the maintenance of the various attitudes and postures. If the cerebellum be the regulator of these locomotive actions, it seems reasonable to suppose that these columns, which are so largely connected with it, each forming a large proportion of the fibrous matter of each crus cerebelli, should enjoy a similar function, and that, as they are the principal medium through which the cerebellum is brought into connexion with the cord, it must be through their constituent fibres that the cerebellum exerts its influence on the centre of innervation to the lower extremities and other parts concerned in the locomotive function, and on the nerves distributed to these parts.

"The nearly uniform size of the posterior columns in the different regions of the cord, whilst it may be noted as unfavourable to their being viewed as channels of sensation, may be adduced as a good argument in favour of their being concerned in locomotion and acting as commissural fibres. It is a fact worthy of notice that these columns experience no marked diminution in size until the large sacral nerves, which furnish the principal nerves of the lower extremities, begin to come off. The reason of this is probably because the fibres of these columns connect themselves in great part with the lumbar swelling of the cord, and some of them, perhaps, pass into the sacral nerves.

"The following remarks will serve to explain the manner in which the posterior columns may contribute to the exercise of the locomotive function. In examining a transverse section of the cord in the lumbar region, we observe a great predominance of its central gray matter; the posterior columns appear large, and the antero-lateral columns seem inadequate in proportion to the large roots of nerves which emerge from it. Now, an analysis of the locomotive actions shows, with great probability, that they are partly of a voluntary character, and partly dependent on the influence of physical impressions upon that segment of the cord from which the nerves of the lower extremities are derived. There are two objects to be attained in progression, namely, to support the centre of gravity of the body, and to propel it onward. The former object is attained by physical nervous actions, the latter by mental. The support of the centre of gravity of the body requires that the muscles of the lower extremities, the pillars of support to the trunk, should be well contracted in a degree proportioned to the weight they have to sustain. The contraction of these muscles seems well provided for in an arrangement for the development of nervous power by a stimulus propagated to the centre, and then reflected upon the motor nerves of these muscles. The stimulus is afforded by the application of the soles of the feet to the ground; it is, therefore, proportionate to the weight which presses them downwards. It is well known that reflex actions are more developed in the lower than in the upper extremities, and the surface of the sole of the foot is well adapted for the reception of sensitive impressions. No object can be assigned for this peculiarity, unless it have reference to the locomotive actions, and the great development of the vesicular nervous matter in these regions betokens the frequent and energetic evolution of the nervous force. All the structural arrangements necessary for this purpose are found in the antero-lateral columns. The posterior columns come into exercise in balancing the trunk and in harmonizing its movements with those of the lower extremities.

"Some support is obtained for this view of the function of the posterior columns from the phenomena of disease. In many cases, in which the principal symptom has been a gradually increasing difficulty of walking, the posterior columns have been the seat of disease. Two kinds of paralysis of motion may be noticed in the lower extremities, the one consisting simply in the impairment or loss of the voluntary motion, the other distinguished by a diminution or total loss of the power of co-ordinating movements. In the latter form, while considerable voluntary power remains, the patient finds great difficulty in walking, and his gait is so tottering and uncertain that his centre of gravity is easily displaced. These cases are generally of the most chronic kind, and many of them go on from day to day without any increase of the disease or improvement of their condition. In two examples of this

variety of paralysis I ventured to predict disease of the posterior columns, the diagnosis being founded upon the views of their functions which I now advocate; and this was found to exist on a post-mortem inspection; and in looking through the accounts of recorded cases in which the posterior columns were the seat of lesion, all seem to have commenced by evincing more or less disturbance of the locomotive powers, sensation being affected only when the morbid change of structure extended to and more or less involved the posterior roots of the spinal nerves."—*Cyclopædia of Anatomy and Physiology*, vol. iii. p. 721*a*.

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*On Diseases Peculiar to Women, including Displacements of the Uterus.* By HUGH L. HODGE, M. D., Professor of Obstetrics and Diseases of Women and Children, in the University of Pennsylvania. With original illustrations. Philadelphia: Blanchard & Lea. 1860. Large 8vo, pp. 469.

THE city of Philadelphia, it appears, is possessed of three colleges, viz., the University of Philadelphia, the Jefferson Medical College, and the Pennsylvanian College. The late eminent Professor of Obstetrics, of the Jefferson Medical College, during his long and brilliant career, shed considerable lustre over the medical school, not only of Philadelphia, but of America at large; and no school of medicine was better acquainted with or more fully appreciated his writings, than that of this country. The late Dr. Meigs was truly no ordinary physician; remarkable not only for his learning, his great practical experience, and the lucid manner in which he expressed his ideas; but also for the high tone tone of morality, both professional and general, which pervaded his life and actions. None could have read his works without coming to the conclusion, that in him was concentrated every quality to render him the beau ideal of the experienced physician, the scholar, and the Christian gentleman.

The University of Philadelphia is at present as highly favoured, as the Jefferson College was some little while ago, in the possession of a professor of obstetrics, by no means unworthy to be ranked as the brother and friend of the late Dr. Meigs. Dr. Hugh L. Hodge was the college companion of Meigs, and his fellow-labourer in the great city of Philadelphia, where they had, up to the year just passed, "toiled *together* for upwards of forty years in the arduous and responsible duties of their profession." It does not astonish us to hear from Dr. Hodge, that "the chain of their friendship had never been

broken, or even sullied;" for, though we never had personal acquaintance with Meigs, yet we can well conceive, how difficult it would be to break a friendship with such a man, even for a single moment. It is no unworthy introduction, this, of the author's, for a man whom Meigs could take into his confidence and urge "to make a sign to posterity," must have evinced to him the capability of doing so and the worthiness to undertake the task.

The result of Dr. Meig's persuasions has been the volume before us, which, on perusal, we have found to be unlike the generality of books of this nature now-a-days met with. It is not a compilation, or book of excerpta, clipped from the works of others, but one consisting of "the results of his own observations and reflections towards the improvement of medicine." Such a book is at least refreshing; but Dr. Hodge's shall be found useful and instructive, as well. The volume is divided into three parts; the first consisting of "diseases of irritation;" the second of displacements of the uterus; and the third, of diseases of sedation. By the latter term is meant inertia. One great object of his work "is to maintain that in many cases the independence of nervous disease is complete; and also that when complications exist, demanding therapeutical assistance, the neurotic affection is often of primary and essential importance, and demands the chief attention of the practitioner." Making nervous irritation the main-spring, he goes on the old axiom, "ubi irritatio ibi affluxus," and thus accounts for the subsequent occurrence of congestions, inflammations, &c. But that the reader may have a better idea of his views, we give the following passage in the author's own words:—

"The hysterical, or the neurotic diseases of women, are therefore states of irritation of the cerebro-spinal nervous system in whole or in part. They are very evanescent when the cause is transitory, as in nervous affections from moral causes, such as fear, anger, anxiety, joy, grief; or from physical causes, as from indigestion, flatulence, &c.: but very persistent in all cases where the cause remains operative, and will then often defy the best directed remedial agents for months and years. Hence the indomitable character of various nervous or neuralgic diseases; the cause is persistent, perhaps it has not been detected, or cannot be removed. Tonics, nutritious diet, exercise, travelling, as well as antispasmodics, narcotics, and stimuli, prove useless, or perhaps worse than useless, and at best are but temporary palliations. The symptoms will return, often with increased vehemence. The location of the cause may be in any tissue or organ of the body. Wherever the irritability or sensibility of a part is disturbed, thence, as from

a focus, may radiate nervous excitements. They are usually perceived in the nerves involved, first at the point irritated; then, if sufficiently severe, in the sentient extremities of such nerves, or in the reverse direction towards their origin in the spinal marrow or the brain, and thence again by reflex influences in any tissues of the economy; thus causing a disturbance of the functions of such tissues or organs, so far as these functions are dependent on nervous influences. In other words, 'nervous irritations' of the brain and spinal axis, and of their dependencies, may, and often do, arise from any local irritation. It is often of small consequence what may be the character of such local irritation; whether it be simply nervous or inflammatory, sympathetic nervous irritations may result in different and often in very distant portions of the animal economy.

"Odontalgia and otalgia often excite neuralgic pains and spasms of the nerves and muscles of the face and neck, and sometimes severe headaches, and other cerebral disturbances. Often croup, asthma, and palpitations of the heart, as well as gastrodynia and colics, are excited by indigestion; and by the same cause headaches, delirium, and convulsions, are very frequently induced. Hysteric convulsions and epileptic spasms, as well as the more dangerous tetanic affections, are constantly excited by the presence of a thorn in the flesh, by a local painful inflammation, or other organic irritations. The proof seems positive to the medical philosopher, not merely that such nervous disturbances follow local irritations, but that they disappear almost instantaneously on the removal of such affections, while they remain obstinate under the best general treatment if the local irritation be allowed to continue. The continuity of tissue, and the wonderful unity of the nervous system, together with the inconceivable rapidity by which impressions are transmitted in health from the circumference to the centres, and from the centres to the circumference, afford a most satisfactory explanation of the transmission of such morbid irritations, and of their reflex influence to distant parts, or to the whole of the cerebro-spinal apparatus.

"A comple history of nervous diseases would involve, therefore, an account, if it were possible, of the innumerable causes, whether intellectual, moral, or physical, which may disturb the sentient system. The nervous and cerebral symptoms being, after all, not strictly the disease, but the *indices*—as they are the results of the primary impressions of the local irritation. Such a task would be endless, and in a great degree fruitless. My object is far more humble and limited. It is to illustrate the general truths already enunciated, by a detailed account of those nervous disturbances which are excited, or kept up by the uterus and its appendages when in a state of morbid irritation, and hence to deduce the proper pathology and therapeutics of irritable diseases in women.

"The general principles applicable to all the variety of neurotic complaints will, it is trusted, be thus evolved and elucidated, inasmuch as no organ of the body has so constant and direct an intercommunion with the nervous system, as the uterus; and inasmuch, also, as the peculiar physiological and pathological character of the female sex is most intimately and indissolubly connected with this organ and its appendages."

So far, then, as we can follow the author in his reasonings —and to us, in some passages, he is rather obscure; we conceive he makes one prime or original starting point for uterine disease—viz., "irritable uterus," which diseased condition has reference, not to its circulating system, and, of course, not to its organic life, but to its nervous system, its animal life. "It has passed from a healthy to an unhealthy state. As regards its nervous system, it is morbidly sensitive; it is irritable." The organic actions are not affected, and there is no alteration of structure, even after the lapse of years. This, then, is, doubtless, a very good definition of irritable uterus, and the disease itself is admirably described; but all other affections of the uterus, which we have been heretofore in the habit of looking on as distinct diseases, our author includes under the head of complications of irritable uterus, except such as arise from mechanical irritation, or the like. Irritable uterus is the chief disease, then we have inflammation in all its forms, tumours, cerebro-spinal irritation, reflex influences, and menstrual disturbances, all, as complications of that primary state of the organ.

Although the author is, to our mind, a little confused in laying down his reasonings, yet there is much ingenuity apparent in his endeavours to prove the soundness of his views. Irritable uterus is certainly his hobby, and he uses it, perhaps a little too hard. It must be remembered, however, as above alluded to, that he considers inflammation is sometimes the original disease, and then irritable uterus is secondary or consequential. Still irritable uterus must be brought into the case, whether primarily or secondarily.

In dealing with what he calls the complications of irritable uterus there is much matter to be commended. For example, he says, "in chronic cases of endometritis"—a term by which he designates inflammation of the uterine mucous membrane—"the mucous tissue,—

"particularly at the lips of the os uteri, loses its natural smooth character, becomes slightly prominent, and rough as well as red; precisely as in similar chronic inflammations of the mucous covering

of the eyeball, 'the tunica conjunctiva;' there by the surgeons termed 'granular conjunctivitis,' or a 'granular condition' of the tissue, from its great similarity to the appearance of the granulations of a simple healing ulcer. This 'granular inflammation' of the neck of the uterus, according to my experience, is not very frequent; but by others has been reported as exceedingly common. It has been described and exhibited as 'ulcers' of the uterus, and, considered either by itself or in connexion with its cause, the inflammation of the membrane, as the real source of mischief and as giving origin to all those symptoms which have been in this work attributed to 'irritable uterus.' This, however, is not proper ulceration. There is an exfoliation of the epithelial scales, a development of the papillæ of the basement membrane with more or less turgescence and elevation of the inflamed surface; but there is no 'solution of continuity' in the tissue, no depression, no loss of substance, no generation of new material—as in a granulating ulcer. And, when recoveries ensue, there is no cicatrix, no thickening, no contraction of the tissues, and no consqeuent alteration in the form of the part affected. At the os uteri, the mucous membranes and the parts adjacent, precisely as in analagous states of the conjunctiva of the eye, return to their original normal state, and the tissues are as perfect as if they had never been inflamed. I have never seen an ulcer of the uterus a proper 'solution of continuity' of the basement membrane, *except* as the result of contusions, or wounds, or of peculiar, specific, or malignant disease. All this may be regarded, perhaps justly so, as a debate as to the meaning of the word 'ulcer.' But 'words are things;' at least they ought to represent things and facts correctly. The Hunterian definition of an ulcerated surface (as intimating a solution of continuity, a destruction of tissue) has been so universally and so long received by the profession, that to extend its employment to a condition of the tissues where there has been no destruction of tissue, is to confuse the minds of practitioners as well as of students, to convey incorrect ideas of the views of the writer, erroneous pathological notions, and to cause manifest and mischievous errors in practice. All these evils, and much greater, in my judgment, have resulted from the application of the word ulcer to such inflamed surfaces of the uterus. The bad influence on the mind and imagination of suffering nervous women is by no means the least of these evil consequences. Ulcers with them are associated with the idea of destruction, of an 'eating' process, of something dangerous, intractible, malignant. And there have not been wanting those in and out of the profession who have magnified these dangers and stimulated the excited mental sensibilities of the patient for their own selfish, sordid purposes. Empiricism and ignorance have gained a rich harvest from this apparently trifling source. Often, very often, have I examined patients, who were reported by physicians and others to have ulcers where none could be detected, or to have had them where no vestige of them, no hardening, no irritation, would possibly be found."

We regret to say that, even in this country, the speculum has been abused; and we agree with Dr. Lee, of London, that nearly everything of importance can be decided without it. It should be remembered, as the author endeavours to impress on his readers, how small a portion of the uterus can be seen, and how it is virtually a viscus surrounded by other viscera, and all but removed from the possibility of ocular investigation. And we would add, when the little that can be seen is brought into view, how very seldom is anything of importance (the commencement of malignant diseases excepted) observed, except what has been wrongly termed "ulcer of the womb," the mere abrasions or granulations above alluded to, the harvest of the unprincipled.

In the section on displacements, the author gives us excellent descriptions of the various uterine dislocations, together with their respective diagnosis and the appropriate treatment for each. Though we have not observed much of novelty in this portion of the volume, still it is characterised by an originality and clearness of description which renders it most readable. Besides, there are several well-executed diagrammatic wood-cuts, illustrating in a most happy manner the anatomy of uterine malpositions, and the mechanical appliances for the treatment of each kind of displacement.

Amongst the various mechanical supports which have been suggested from time to time to combat these displacements, that for the cure of retroversion as invented and employed by Dr. Simpson, of Edinburgh, has not been forgotten. It will be remembered by our readers that this consisted of an external frame-work, a vaginal stem and pessary, and a style to project into the cavity of the neck and body of the uterus, and that this instrument, when adjusted, was worn for a considerable period. It will also be in the recollection of many, that the use of this instrument was severely commented on by Dr. Lee, of London, some time ago, and was christened by him "the uterine impaling machine"[a]. Bearing this in mind, it may be interesting to hear the opinions of the Philadelphia Professor on this instrument. We shall quote him at length. He says—

"Professional attention has been turned to this 'intra-uterine pessary,' as it is termed, by the talented but enthusiastic Professor in the University of Edinburgh, Dr. Simpson, with whose valuable suggestions on many subjects the profession are familiar. His

[a] Clinical Reports on Ovarian and Uterine Ovaria. Robert Lee, M. D. London: 1853.

course is, however, to be followed like that of most pioneers, 'cum magnâ prudentiâ longo intervallo:' especially in the employment of this instrument, very efficient in restoring or fixing the uterus in its normal position, but fraught with many dangers."

Again:—

"There are, we think, so many serious objections—theoretical and practical—to be urged against this apparatus, that it would seem to be impossible that it should come into general use. Its employment will probably be confined to special cases, or restricted to those few individuals who are so exact in their diagnosis as to estimate aright the peculiar cases to which it is adapted, and so cautious in their practice, as to watch their patient day after day, not to say hour after hour.

"One important objection to the instrument, as proposed, is the oxidizable character of the metals employed—German-silver, or a mixture of silver and nickel. Dr. Simpson has also employed lead, and copper, for the vaginal disk. These metals, from the moisture, atmospheric air and heat in the vagina, will oxidize with more or less rapidly, and thus be corroded; and their oxides, combining with the acid secretions of the tissues, may become more or less irritating. The effect, if any, in preserving the metals in their natural state which may arise from any galvanic influence due to the combination of different metals, must be decided by the chemist and by observation.

"The second objection is from the frequent necessity of employing napkins, tapes, and bandages, with all their uncleanliness and frictions, especially in warm weather, with the absolute necessity of repeatedly changing them, as have been specified when speaking of external supporters.

"The external frame-work itself is a great objection, rising up in front of the sensitive tissues of the nymphæ, clitoris, and labia, and liable to produce more or less friction and pressure; its mobility is, with all care, often so great, that it cannot be worn without additional applications, as tapes or bandages.

"The vaginal stem must excite more or less irritation against the urethra, nymphæ, and other sensitive tissues, which few patients will bear.

"The fixed character which all vaginal stems give to pessaries has already been pointed out as a most serious objection. They never yield to pressure from above. The internal irritable tissues are pressed against them forcibly at all times when the patient is erect, and often violently in coughing, vomiting, sneezing, &c. In the case of an intra-uterine pessary, this objection is stronger than in all others, as the points of internal resistance are, first, the mouth of the uterus; secondly, its internal surface; and thirdly, in some cases, the upper or transverse side of the triangular cavity of the uterus, directly against the point of the intra-uterine stem.

"The chief pressure is received against the orifice of the uterus, and the lower extremity of the cervix, which rests on the vaginal disk—a fixed point. In all the impulses from above, the impression is made on these delicate and irritable tissues, so that sometimes the orifice is even dilated by the pressure against the disk. It has already been pointed out in detail, that the results of such pressure in neglected cases of prolapsus uteri, against even the soft parietes of the vagina, are flexion, soreness, nervous disturbance, pain, inflammation, continuing for years, with the granular, and sometimes, it is said, even with the ulcerated condition of the mucous membrane. It was also shown that such inflammations of the os were aggravated by the ball, the concave disk, the double and plano-convex pessaries, even though they were movable in the vagina. The theoretical conclusion, therefore, is clear, that more irritation would ensue when the disk is held immovable, as in the intra-uterine pessary, against the orifice by means of the stem.

"I experimented several times with the intra-uterine stem and disk, generally in women whose catamenia had ceased, and where no marked sensitiveness existed in the uterus. In each, after a few days, leucorrhœal and bloody discharges ensued, with considerable pain and irritation, sufficient to indicate the propriety of discontinuing the instrument.

"The intra-uterine probe is said to be worn with impunity. The testimony given will never be acted upon without great caution, if we reflect upon the sensibility of the internal tissues of the uterus, so great indeed that a uterine sound can seldom be introduced, especially through the internal os uteri, without causing severe pain, sometimes agonising, if we may judge from the exclamations of the patient, and from the intolerance of the uterine cavity of fluids, simple as well as stimulating. The vascularity and delicacy of the lining membrane are so great, that probing the uterus is generally followed by the effusion of blood; and the slightest mental, as well as physical excitement, will often produce leucorrhœal and menorrhagic discharges.

"In the case of the intra-uterine pessary, the internal stem is not merely always in contact with these sensitive vascular tissues, but presses upon them more or less firmly during every motion of the body, as this stem constitutes the means by which the organ is kept in position. It is not, therefore, to be wondered at that in irritable and congestive, as well as inflammatory, states of the uterus, this instrument, as is acknowledged, cannot be borne, or that it has sometimes to be removed because leucorrhœal or menorrhagic discharges are excited or increased. The greater wonder is that such irritations and discharges are not the constant and unavoidable result in all cases. That such is not the uniform effect can only be explained by that law of vital tissues by which, under constant pressure, if not too severe, parts become gradually more insensible to irritations.

"The most serious pressure is, however, to be apprehended from the point of the instrument against the internal upper extremity of the cavity. Dr. Simpson has attempted to obviate this danger by making the instrument shorter than the long axis of the cavity. The internal stem measures two inches and one-third, while two inches and a half is the usual length of the uterine axis internally. The difference is merely two lines, or one-sixth of an inch! What possible security can be given that, in the various motions, this small space of one-sixth of an inch will not be obliterated, and that the fundus will not impinge against the point of the probe? If the uterus were perfectly firm—as if made of metal, of bone, or even of cartilage—such an accident could not perhaps occur; but the tissue of the uterus, although firm, is flexible, and bending of the organ is a fact of daily observation. With an intra-uterine supporter there can be, it is true, no marked lateral, anterior, or posterior flexion, the metallic rod within resists such changes; but the whole superincumbent pressure must force the fundus downwards, and as the opposite extremity of the organ is fixed by the vaginal disk, the yielding of the uterus will, therefore, be from above downwards, in the direction of its length: all such yielding will endanger the forcible impinging of the uterus against the point of the internal stem. This danger is aggravated by the gradual dilatation of the os, and the eversion of its lips, so that the stile penetrates still deeper into the organ. Either of these circumstances—and, of course, still more when both are in unison—can readily shorten the perpendicular diameter of the uterus one-sixth of an inch, when mischief of a serious character would ensue—inflammation, ulceration, even, it may be, perforation of the uterus, and peritonitis with its dire consequences. Such results are said to have occurred, and certainly are to be apprehended, especially as few are expert enough to measure and arrange the safe proportions which ought to exist between the length of the stile and that of any individual uterus. Dr. Simpson speaks of using one some three and a half inches long in a hypertrophied organ with impunity; but one of our western physicians, less skilled in diagnosis, inserted an intra-uterine pessary, with a stem five inches in length, into a uterus whose length was found to be but three inches when measured by the uterine sound. The instrument is in my possession.

"This particular danger can easily be lessened in a great degree by diminishing the length of the stem, so as to allow half an inch or six lines between the internal surface of the fundus and the point of the stem. Nevertheless, the strong objections of the uterus being firmly fixed by an immoveable pessary, and the irritation arising from the intra-uterine stem, &c., must be regarded as of serious import. Minor objections may be alluded to, such as the occupation of the vulva and vagina so as to prevent the natural uses of this canal; and the irritations and discharges which the vaginal stem would usually produce."

We are certain that such an instrument could never come into general use in this country, and we are glad that the author has given such a guarded, in fact, we may say unfavourable, opinion, of it. But we cannot agree with him, that "further reflections and experience may ameliorate the objections;" he has detailed, or "may suggest modifications in the instrument which may render it more safe, and not impair its efficiency." Under all circumstances, we object to the introduction of metallic implements into the cavity of the uterus, and more especially to their maintenance there for an uncertain and, in some cases, an unlimited period. So that we do not expect much benefit to arise, even from the author's modifications of Simpson's "impaler;" nevertheless, we feel it to be our duty to give insertion here to his description of it:—

"*Simpson's Intra-Uterine Pessary modified.*—I venture to present a drawing of the modification, just alluded to, of Dr. Simpson's intra-uterine pessary. As will be observed, a vaginal elliptical ring was substituted for the external frame-work, and the intra-uterine stem made shorter. The experiment, as regarded the maintenance of the uterus in position, was a perfect success, and at the same time more mobility was allowed to the organ under the muscular efforts of the patient, and all the irritation and annoyance due to an external apparatus were done away with.

"The intra-uterine stem was made but two inches in length for a uterus measuring two and a half inches internally. The vaginal disk had attached to a point in its circumference a short stile four lines in length, with a small foramen at its outer extremity. The disk with its stile should make, with the intra-uterine stem, an acute angle, about 40° to 50°, corresponding somewhat to that made by the axis of the uterus with that of the vagina. An elliptical ring was then made, slightly curved in its length, and two inches and a half long by two inches broad (but of course the size must vary with the size of the vagina), and with a perforation in a longitudinal direction, through the bar at one extremity to receive the stile.

"Before *introduction*, a strong thread or a fine flexible wire is to be passed through the eye in the vaginal stem. The pessary is then passed into the vagina, the stem carefully introduced into the cavity of the uterus, and the organ may be partially elevated. The string, hanging out of the vagina, should now be carried through the opening in the extremity of the ring. The ring is then introduced into the vagina, with its convexity toward the bladder, and the string being made somewhat tense, acts as a director, by which the ring can be pushed so as to cause the vaginal stem to enter the foramen in the ring. The string is now to be withdrawn; the pressure of the vagina, &c., will keep the ring firmly fixed on the stile, so that the direction of the intra-uterine probe represents

that of the axis of the uterus, and the pessary that of the vagina. If the uterus be not perfectly restored, a little pressure on the ring will accomplish this purpose; and the anterior wall of the vagina under the influence of the superincumbent weight of the viscera, increased by muscular effort, will constantly depress the pubic extremity of the ring towards the perineum, and thus compel the uterus under the command of the stem to follow its motions. All these tissues being pliable and elastic will allow a certain degree of yielding of the uterus to the distention of the bladder, bowels, &c., and thus the instrument does not materially interfere with the mobility of the womb.

"The *removal* of this instrument is very readily accomplished by drawing the elliptical ring towards the orifice of the vagina, which detaches it from the stile, after which the vaginal disk, with the intra-uterine stem, can readily be removed. If by any arrangement of this kind too much lateral motion should be allowed to the uterus from the cylindrical form of the stile in the vaginal disk, this can readily be prevented by making it and its sheath in the extremity of the ring, flat, so as to prevent rotation.

"Whatever modifications, however, may be suggested, by which the evils are moderated, yet the intra-uterine pessary ought to be regarded with great suspicion, and be very cautiously employed, even in the very limited number of cases to which it is at all applicable."

We fully concur with the author's expressions conveyed in the last sentence.

The volume concludes with a few pages on what the author terms "diseases of sedation," which term is tantamount to "inertia," an older and much better word. But "sedation," we presume, has the charm of being somewhat allied to the word "location" (one frequently made use of throughout the work), so that we shall take "sedation" to be "inertia" Americanized. The chief feature of this section is, of course, amenorrhœa and its concomitants; and though short, yet it is a good dissertation on the disease, containing an excellent *resumé* of all that is known theoretically and practically on the subject.

The great worth of Dr. Hodge's book is, that it conveys the experience of one long engaged in the practice of obstetric medicine; and though as a work it is decidedly inferior to that which emanated from his revered colleague Meigs, still it is in every way worthy of the school of Philadelphia, long since rendered famous by the eminent Professor of the Jefferson College.

*Diagrams of the Nerves of the Human Body.* By W. H. Flower, F. R. C. S. E. London: Churchill, 1861. Folio, pp. 8. With 6 Plates.

This is a very timely publication, and one of equal use to both the student and the practitioner. The difficulty of acquiring a thorough knowledge of the nervous system is only equalled by the difficulty of retaining the knowledge when it has been acquired, as our readers must themselves but too well know; and therefore we are certain they will gladly avail themselves of the assistance to the memory which Mr. Flower's beautifully executed plates so fully affords. In five large folio plates—

"the distribution of all the nerves of the body, so far as the branches have received distinctive appellations, is shown; and their divisions are traced to the muscles and to the various regions of the cutaneous surface. To afford greater facility for reference, the names of the muscles are printed in red letters, those of the nerves being black. It must be clearly understood that the plates are only diagrams, or plans; and that in reducing to a plane surface objects which are in reality superimposed at various distances, and which sometimes cross one another, their mutual relations and proportions must often be disarranged."

The eye, however, after a little study, becomes accustomed to this necessary mechanical arrangement; and to all who have acquired their knowledge of the anatomy of the nervous system by dissection, is productive of little inconvenience. It would be far different, however, were it attempted to teach the student in the first instance from these plates; such a mode of study could but lead to erroneous ideas of the nervous distribution, and to confused notions. We therefore recommend the use of these plates to advanced students only, and to the practising physician and surgeon; and to these we warmly recommend their employment both for reference and for study. As might be expected from the position the author holds as Demonstrator of Anatomy at the Middlesex Hospital, they are carefully and correctly executed; and we cannot bestow too much praise on the lithographer, the printer, and the publisher, for the manner in which Mr. Flower's drawings have been presented to the profession, and his ideas carried out.

*Ten Lectures, Introductory to the Study of Fever*. By ANDREW ANDERSON, M. D., Lecturer on the Practice of Medicine in Anderson's University, Glasgow. London: Churchill. 1861. 8vo, pp. 180.

THERE is nothing more difficult for either an author or a reviewer than to estimate justly what is the precise amount of elementary character to infuse into a work which is professedly only an introduction to the study of the subject-matter of which it treats. The work before us illustrates the difficulty in question in a remarkable degree. It is evidently the production of a man of learning and practical experience. But we are more than doubtful as to its utility in the hands of persons not already pretty well versed in fever pathology. Dr. Anderson's lectures will be read with interest and profit by men who have seen and treated fevers. How far they are so framed as to supply to the student the *prolegomena* necessary for entering on the consideration of special fevers subsequently, is what we are somewhat sceptical about.

How, for example, can it be supposed that the brief and cursory notices of the general pathology of fever so lightly and passingly sketched in the first chapter, furnish sufficient of even the most elementary knowledge on the profound questions involved. And yet it is impossible to glance through this section without a conviction that the writer is well versed in what he so hastily skims over; and a regret rises to the mind that an author who promises so well has not achieved more. On the classification of fevers Dr. Anderson seems to share the uncertainty of most of the fever pathologists of the day; and his arrangement and grouping of the several varieties of fever are evidently to be accepted as only provisional. But besides a classified view of the several distinct fevers as such, the author, "speaking of fever irrespective of any particular poison," recognises a division of fever in general into eight different TYPES—"in other words, eight distinct phases which fever may assume." Without more precise definition than the author has furnished, we fear that a category of types and a category of classes can hardly fail to confuse the student. What it is intended to convey is, evidently, that *any* fever of *any* class may present this or that type of febrile action, according to the condition of the constitution for the time being, and the causes which have called the fever into play. Thus a case of scarlatina, measles, or small-pox, may present a "mild," or "toxic," an "inflammatory" or an "asthenic"

type. The author recognises eight different types, viz., the "mild," the "toxic," the "congestive," the "inflammatory," the "asthenic," the "nervous," the "septic," and the "typhus."

These several types are discussed at some length in the earlier portion of the second chapter; but here again we have to complain of the author's inadequate treatment of an important subject which he manifestly understands well. The same must be said of the pages devoted to the "complications," "sequence," and "management" of fever, in all which sound doctrine, based on practical knowledge, is fully evidenced; but the treatment of the subjects is too rapid and generalising for the uninitiated, and for the most part, simply unnecessary for those who have themselves investigated the pathology of fever.

The classification of fevers adopted by the author would appear, as we have before stated, to be of but a provisional nature. It is as follows:—

I.—*Simple Fevers:*

1. Ordinary ephemera and synocha.

II.—*The Typical Eruptive Fevers:*

2. Chicken-pox.
3. Small-pox (Cow-pox).
4. Scarlet fever.
5. Measles.

III.—*The Imperfect Exanthemata, or ordinary continued fevers of this country:*

6. Typhus.
7. Enteric fever.
8. Gastric fever.

IV. *The Bilious Fevers:*

9. Relapsing fever.
10. Malarious fever.
    1. Intermittent fever.
    2. Remittent.
11. Pestilential fever.

V. *The Phlegmonous Fevers:*

12. The plague.
13. Puerperal fever.
14. Erysipelas.

VI. *The Adynamic Mucous Fevers:*

15. Diptheria.
16. Influenza.

The arrangement is one open to objection in many points of view; but it has long been our opinion that in the present position of nosology, any simple categorical exposition of the fevers, practically recognised as distinct, is to be accepted as such on the responsibility of the author who employs it. A strictly scientific classification of fevers seems for the moment unattainable.

Not a few will be disposed to quarrel with Dr. Anderson for his *pronounced* opinions on the subject of contagion. His views on this subject break forth in several places; but in none more strongly than when, speaking of the exanthematous fevers, he says:—"All these fevers are contagious—all of them *very* contagious, especially towards convalescence; and there is no evidence that they ever arise from any other source than this—each from a previous case of the same disease." It may be asked, had Cain or Abel small-pox? and if so, following the Linnæan theory, are we to assume that this, and similar entities of diseases, begotten like from like, have been transmitted in unbroken parallel lines, from prototype diseases engendered amongst the first children of Adam? Or, on the other hand, are we to seek for the development of the numerous species of diseases from a common morbid state, from which all those diseases now existing have branched in divergent lines?

In speaking of small-pox, the author cites from the private letter of a friend the following remarkable and interesting passages, corroborative of the belief now entertained by many, that forms of disease identical with those which prevail in the human subject are not by any means so unknown amongst the lower orders of creation as was at one time supposed.

"In the year 1841, I was in the province of Veragua, in New Granada, to the north of the Isthmus of Panama, and left the town of St. Jago, on the western coast of David, in Chiriqui, a town in the interior, about sixty or seventy miles to the N. E. (and leeward) of St. Jago.

"The small-pox was raging with great violence in St. Jago, but there was no appearance of it in David. A few days after my arrival there, taking my customary morning's ride in the forest, which teems with animal life, I was struck by observing one or two sick and apparently dying monkeys on the ground under the trees. The next morning I was struck by the same singular appearance (for it is very unusual to find a wild animal sick; they instinctively hide themselves), and by thinking that I perceived several on the trees moping or moving about in a very languid and sickly manner. I consequently dismounted, and carefully examined two which were

on the ground—one dead, and the other apparently dying; and after careful examination, no doubt remained on my mind that they were suffering and had died from small-pox. They presented every evidence of the disease; the pustules were perfectly formed; and in one instance (that of the dying one), the animal was nearly quite blind from the effects. A few days afterwards (I think about four or five days) the first case of small-pox appeared amongst the inhabitants of David; and in the course of a fortnight, one-half of the population was stricken."

This observation is one of much interest and value. It illustrates the old law so well and distinctly recognised by the master-mind of Homer, that epizootic disease precedes the epidemic invasion of the human race:—" Ουληας μεν πρωτον επωχατο και κυνας αργους," as the Coan bard narrates of the plague sent by the wrath of Apollo upon the Grecian forces.

Passing over the sections on scarlatina, typhus, and enteric (typhoid) fever, which offer little for remark except their extremely light and cursory character, our attention is arrested by the heading, " Gastric Fever," which the author believes—

" to be a separate disease, on the like ground to that on which I hold enteric fever to be different from typhus. * * * The affection of the stomach distinguishes it from enteric fever, while that of the bowels is quite different from the follicular lesion proper to that disease, but to which there is here no tendency, though there is often very great irritation of the mucous membrane going on even to ulceration; and we have a diarrhœa, but quite unlike that which I have described as occurring in enteric fever. Gastric, like enteric fever, I believe to be contagious, though in a less degree than the proper exanthemata; and, like all other fevers, it may prevail epidemically. More than most it is, I believe, connected with what early in these lectures, we called 'effluvia,' emanations, that is, from decomposing animal matter, which seem to me sufficient of themselves to produce the disease; and it often assumes more or less of a remittent character, which is not observed in the fever we have hitherto been describing. Its *eruption* is the most obscure of the three which belong to the imperfect exanthemata, and also the most inconstant. It does not appear, like that of typhus, over the whole body, but like the spots in certain pores only on the epigastrium and abdomen; yet, unlike them, it is not elevated nor pink. It consists of a few small, insignificant-looking red spots, which you would probably not observe unless you looked for them, and which appear about the seventh day of the disease."

Dr. Anderson describes this gastric fever as " most irregular in its stages, form, degree, and symptoms;" and we submit it

as worthy of consideration, whether it may not be with advantage regarded as an errant type of "enteric fever," rather than as a distinct affection. At all events, we must consider the author's case for this fever as a disease distinct on the one hand from enteric or typhoid, and on the other from the gastric remittents of childhood, as "non-proven." Some brief account of yellow fever, and also of the "mucous fevers," diphtheria, and influenza, concludes the work; but these sections demand no special notice at our hands. With these observations we must close our notice of Dr. Anderson's Lectures. We have perused them with interest, and not without profit; but we could yet wish that he had dealt with such important topics in a more full, comprehensive, and exhaustive manner.

# PART III.

## MEDICAL MISCELLANY.

### TRANSACTIONS OF THE COUNTY AND CITY OF CORK MEDICAL AND SURGICAL SOCIETY[a].

(*Continued from* p. 255.)

SESSION 1860-61.

JANUARY 23, 1861.

DR. BAINE in the Chair.

*Cases of Lithotomy operated on in the Cork South Infirmary.*—By WILLIAM KEARNS TANNER, M.D., L. & F.R.C.S.I., President of the Society; Senior Surgeon, Cork South Infirmary; Surgeon, Cork Lying-in-Hospital; late Lecturer on Surgery, Cork School of Medicine, &c., &c.

Stone in the bladder must be considered as one of the most serious diseases with which the human body is liable to be afflicted.

When considered either in respect to its dangerous nature, the lamentable suffering it causes, or the great judgment it requires to determine upon and rightly select the best means for its cure, and then to accomplish so important an undertaking successfully, is indeed a serious matter. I now intrude on the valuable time of this Society for the purpose of eliciting opinion, and causing a discussion on the most difficult and interesting points which have occurred during my treatment of those unfortunate sufferers afflicted with this dire malady, and for this purpose beg to relate the details of some of my cases, each presenting different characters and a corresponding difference in their treatment.

The first was that of a little boy, aged five years, from near Belgooly, admitted into the South Infirmary on the 10th of March, 1854. He had been afflicted for nearly two years, during which time he was subjected to different plans of treatment. He was

[a] These Reports have been furnished to us by Dr. S. Henry Hobart, Secretary to the Society.—ED.

much emaciated, and had a pale, haggard look. The rational symptoms of stone were well marked. He suffered intense pain at the orifice of the urethra after micturition, and his prepuce was greatly elongated from constantly pulling it. He had occasionally a sudden stoppage of his urine, which he generally was able to relieve by a change of posture; and the torture which he suffered in the back, loins, and perineum, extending on rough motion down the groins and even to the soles of his feet, was not to be described. When he had a fit of the stone, it was accompanied by frequent micturition; and in consequence of the straining, prolapsus of the anus to a considerable extent existed. His urine was alkaline, and contained mucus, with a small quantity of pus, but no trace of albumen could be detected. From the irritation set up in his bladder, causing slight chronic inflammation of its mucous membrane, I ordered him a hot bath, mild laxatives, demulcent drinks, for the purpose of bringing it to a more healthy state before sounding him, which was accomplished after a few days, when I introduced the metallic sound, and distinctly felt and heard it strike against what appeared to me to be a large, but soft, calculus. On consultation with my colleagues, we unanimously agreed that the operation of lithotomy would be the most likely to succeed, in consequence of the youth of my patient and the great size of the stone (though his bladder was then capable of retaining a considerable quantity of urine, and his power of emptying it unimpaired). We accordingly fixed the day but one after, for the operation, and at that time proceeded to accomplish it, having on the previous evening administered a small dose of castor oil, and in the morning given him an enema for the purpose of emptying his bowels, he was then placed in the usual position for the lateral operation of lithotomy; and being properly secured, and having ascertained that his rectum was empty, I introduced a full-sized and deeply-grooved staff (on its convex surface) into his bladder, and having, without difficulty, felt the stone, I gave the staff into the right hand of my assistant, who directed its groove a little to the left side, but still keeping it tolerably well up against the pubes, whilst with his left hand he drew up the scrotum, and made the perineum tense. At this moment my little patient endeavoured to empty his bladder (which he had been prevented doing for some hours previously), and whilst straining caused a considerable prolapsus of the rectum, which I found it quite impossible to keep reduced, and therefore proceeded with the operation, which I commenced with an ordinary scalpel, by sinking it deeply at the left side of the raphé of the perineum, about one inch and a quarter in front of the rectum, and continued my incision, by cutting deeply into the space between the erector penis and accelerator urinæ muscles, and pushing it up, as it were under the pubes, until I struck the staff between the bulb of the urethra and prostate gland, when I gradually drew my knife downwards, outwards, and more towards the surface, until I brought it a little further back than between the tuberosity of the ischium,

and protruded gut, which I protected from the knife, having drawn it aside with my finger; I then felt in the wound with the fore-finger of my right hand for the groove in the staff, which I took from my assistant, and held in my left hand, a course I consider always necessary to take for the purpose of feeling, through the sympathy between both hands, that the groove in the staff is bared in the proper place, and to be a guide in conveying knowledge of the relative anatomy of the parts. I dwell on this point, from the difficulty which I experienced, in one of my earliest operations, in getting into the groove of the staff, but which was quickly and easily overcome by this means. This manipulation occupied but a few seconds; and handing back the staff to my assistant, I guided the knife with the fore-finger of my left hand into the bared groove of the staff, and feeling it securely placed, and depressing the handle, pushed it steadily into the bladder, when the urine flowed; then, having withdrawn the knife with care, I passed my finger easily into the bladder, and ascertained that the wound in the prostate was of quite sufficient size to admit of the easy entrance of the lithotomy forceps; and, what I consider of most importance in this operation, that it did not extend beyond the prostatic portion of the bladder. I then felt the stone, which was enormously large; and my assistant having withdrawn the staff, I introduced the forceps, guided by my finger, with which I then felt the calculus, when, having removed my finger, I easily and firmly grasped it, but the handles of the forceps being very far apart, I feared having caught it in its large diameter, when I let it go, and tried it again, but not with much better success. I then proceeded to use gentle traction, and, as I increased the force, the stone formed a globular perineal tumour, of very considerable size, when I was strongly pressed by some of my friends to enlarge the opening in the left side of the prostate; but I firmly declined to do so, with the approval of my colleague, Dr. Gregg, knowing the danger likely to result from extending the incision beyond the prostatic portion of the bladder; but whilst I was thinking of the total impracticability of removing the stone by the ordinary means, and of what course would be the most judicious for me to take for that purpose, it suddenly crushed in atoms between the blades of the forceps. I then removed some of the larger fragments with the forceps, and afterwards found the scoop the best instrument for getting out the remainder, which was quite pulverised, and its total removal appeared to be interminable. I suppose from twelve to eighteen times I brought out the scoop nearly full. I then washed the bladder well out with the syringe; and, having ascertained that all the remains of the calculus were removed, I passed a piece of gum-elastic catheter through the wound into the bladder, and, having secured it in its place, lightly plugged it with lint, and was indeed glad to have my poor little suffering patient removed to bed, after undergoing an operation which lasted, I believe, for nearly half an hour, and from the effects of which he appeared dreadfully exhausted. I then administered to him an opiate

draught, with some aromatic spirits of ammonia, which I ordered to be repeated again in the evening, and to have his abdomen well fomented; and on the following morning was glad to find that he had slept for nearly four hours during the night, and was in all respects tolerably comfortable. I repeated his medicine, and ordered the free use of diluents, and in forty-three hours removed the gum-elastic tube from the wound, the walls of which I considered sufficiently covered with lymph and solidified to prevent extravasation of urine. At this time he felt some spasmodic pains in the abdomen; his tongue was white, and his skin hot; but his pulse continued good, and under ninety beats in the minute. His bowels were not relieved since the operation, but his urine was abundant, and passing off through the wound, in which he complained of little pain. I ordered him some castor oil, with aromatics, and a little syrup of poppies; and if his bowels were not then relieved, an enema to be administered, containing turpentine and tincture of asafœtida, which I found on the following day to have afforded wonderful relief, having removed from the colon a considerable quantity of scybala. From this time out he gradually improved; the wound healed up, the urine passed through the natural passage, and in a month he was discharged, cured. I have since heard of him, and been informed that he continues in perfect health.

The next case was a boy named Eugene Callaghan, aged six years. He resided in Mitchelstown, and was admitted into the Cork South Infirmary on the 15th September, 1858. His constitution did not appear to have suffered much from this disease, though the symptoms were well marked. On sounding him, the stone did not appear large, but was evidently hard, and his bladder in all respects was quite healthy.

This was a most favourable case for the operation of lithotomy, and accordingly it was the one determined on in this instance; and on the 22nd of September, after putting him under the influence of chloroform, I operated on him in exactly the same way as in the preceding case, and removed from him a uric acid calculus, weighing four drachms and a half. The operation was completed under the minute, without any accident. The patient rapidly recovered, without any unpleasant symptoms, and was discharged cured from the hospital on the 23d of October, being in hospital forty-five days.

The next case was that of a boy, named John Grant, residing in Coach-street, in this city; he was eight years old, and admitted into the South Infirmary on the 14th of October, 1858. On my first interview with him, I was deeply moved to see so miserable a creature; his worn and pallid aspect, his attenuated frame, his great sufferings, his pitiful entreaties for relief, and hopeless expression, were enough to soften the hardest heart; he was then over two years thus afflicted, and his unfortunate family pressed upon me to use any means, regardless of the danger incurred or pain inflicted, which would give a chance of releasing him from his wretched condition. In addition to the other strongly-marked symptoms of stone in the

bladder, this poor creature suffered from a constant dribbling of urine, and pain in the perineum, accompanied by a frequent bearing down; his bowels were irregular, either constipated, or he had diarrhœa, but had not for some period passed a formed evacuation; his urine was phosphatic, and hectic fever was evidently hurrying him to the grave. I endeavoured to pass the sound, which was arrested at the neck of his bladder by striking against a calculus, which seemed to be impacted there; and, on passing my finger into his rectum, I readily ascertained that such really was the case, and that it seemed to extend itself into the bladder, and to be of large size. I had a full consultation on this important case; and, though it seemed most unpromising, resolved, with the sanction of my colleagues, to try and rescue this poor creature. The lateral operation was the one resolved on, as likely to give him the best chance. I was to cut down on the stone, and then to extract it the best way I could. Previous to the operation, I used my best efforts to cheer him up, and by kindness to give him courage; also to improve his general health with nutritious food, and invigorating and soothing remedies. The decoction of Pareira seemed to have good effect in relieving the irritability and pain of his bladder, and on the 24th of October I considered him fit to undergo the operation, when he was placed on the operating table, the usual precautions having been taken, and being secured in the usual way for the lateral operation. It was impossible to get him to retain his urine, and useless to introduce a staff into the urethra. I got my finger into the rectum, and with it tilted forward the calculus, so as to make it be felt in the perineum. I then made an incision as described in the foregoing case, only, instead of striking the staff with the knife, I struck the calculus. I then introduced my finger into the wound, and ascertained how the calculus was situated; and then, withdrawing my left finger from the rectum, and introducing it into the wound, and using it as a guide for my knife, opened the urethra fairly between the bulb and prostate, and keeping it firmly against the calculus, pushed it into the bladder, cutting the left side of the prostatic portion, as in the usual operation. I then, with some difficulty, but at the same time gently, introduced my finger into the bladder, and felt the stone completely encircled by its neck, and extending far into it. I then endeavoured to grasp it with the forceps, when a small bit broke off; and I found it quite impossible to detach it from the membranes, by which it was closely and intimately surrounded. On a few moments of as calm reflection as the circumstances would permit of, I saw that my only chance of success was to push the calculus completely into the bladder, which I at once accomplished without any difficulty, but felt that it would be quite impossible to remove the stone without enlarging the wound; and as I had cut the left side as far as I could fairly do, without injuring the body of the bladder, I considered my best course was to cut the right side to the same extent, and at once did so with my blunt-pointed bistoury, without experiencing any difficulty, or

any unpleasant consequences having taken place. I then introduced the forceps into the bladder, guided by my finger, and at once delivered the stone. I then introduced a gum-elastic bougie into his bladder, as in the former case, and was happy to find that my patient had borne the operation admirably, which lasted four minutes and a half. He got some restoratives during the operation, was then placed in bed, and a suitable anodyne was administered. He was well fomented at night, and had his anodyne repeated; and on the following morning I found him tolerably well, though he had not slept much. He complained of occasional darting pains through the abdomen. I ordered him a few drops of laudanum, with a small quantity of aromatic spirits of ammonia every six hours, and four grains of Dover's powder, and three of hydrargyrum cum cretâ, to be taken at night, and to be fomented again. The urine was passing freely through the wound, in which he did not suffer much pain. On the following morning I found that he had rather a comfortable night, and slept some hours; but his bowels not being affected since the day of the operation, I ordered him a small dose of castor oil, with a little paregoric. In the course of that day I received an urgent message, to say that it was feared my stone patient was suffering from peritoneal inflammation, and in fact was in a deplorable state; when I hastened to his side, and from my former experience of such cases, expressed my opinion to our able and highly satisfactory house-surgeon, Dr. Callaghan, that I believed his ailment to be spasmodic, and dependent on scybala retained in his bowels from the pressure of the stone on the gut, as in some cases of pregnancy familiar to us all. My poor patient, from his weak and irritable state, was quite overcome from the pain; he looked dreadfully sunken, and his skin was cold and clammy; his pulse was 90, small and weak, but regular, and his tongue white and moist. He bore, and rather liked, pressure on the abdomen. Dr. Callaghan quite concurred in my opinion of his case, and we ordered him a dose of the aromatic mixture, and to have a large enema containing turpentine and tincture of assafœtida thrown up. I waited to see the result, and was happy to find masses of feculent matter come away: in fact, his bowels were in a dreadfully loaded state; it was incredible the quantity which he passed. The gum-elastic tube was removed from the wound, a poultice was applied, chicken-broth was freely administered, and he occasionally took purgatives likely to solicit the action of the bowels, and for some time scybala continued to pass off. My patient rapidly recovered, and was discharged, perfectly cured, on the 20th of November, 1858, being forty-seven days in the house.

I have but few observations to make on the foregoing cases. It is generally admitted in this country that lithotomy is a preferable operation to lithotrity for many persons. All appear to agree that the success of the operation, in a great measure, depends on not cutting beyond the prostatic portion of the bladder into the loose areolar tissue, which is sure to be followed by infiltration of urine and its dire consequences: indeed I have seen the most prolonged and

painful operations successful where the bladder was scarcely at all cut, and the calculus was delivered by dilatation of its neck. In my own practice I have been most careful in uniformly limiting my incision to the dense prostatic portion of the bladder, and to which I attribute my uniform success in a not inconsiderable number of cases which have been operated upon by me.

Peritonitis has, by many, been regarded as a frequent occurrence after this operation. I have only seen it take place as a consequence of infiltration of urine; but I have often observed that abdominal pain, dependent on scybalæ in the intestines, has been mistaken for it, and the patient submitted to much unnecessary and injurious treatment. I am happy to say that the old-fashioned idea of few recovering completely from the effects of this operation is now obsolete. In all the cases which I have had, the patients were restored to perfect health after the calculus was removed, and to my knowledge never suffered any ill effects from it to the present time.

Dr. Finn, on behalf of Dr. Rountree, of Ballincollig, exhibited pathological specimens, illustrative of following case, and read the particulars as communicated:—

*Chronic Pleuritis, with Effusion.*—The patient was a young artillery driver, named Barnes, stout and rather muscular, who was brought to the military hospital at Ballincollig on the evening of the 6th December, 1860, apparently in a state of great suffering, from an acute pain in his left side, which was much aggravated by his short and difficult breathing. His countenance was troubled and restless, lips and tongue purplish; he was chilly, and anxious to lie down; his hands and extremities were cold and livid; tongue was coated; pulse hard, quick, and oppressed; he endeavoured to restrain a short cough which came on occasionally, to avoid the pain it caused him. I learned from his comrades that the proximate cause of the present attack was in consequence of having incautiously slept on a heap of wet horse-litter in the open air for some time during the forenoon, which was bitterly cold and damp, and this after a debauch on the previous evening. On awaking, he found himself extremely ill, and shivering with cold and pain. He was not brought to the hospital until late in the afternoon. On examination of the thorax, I found on percussion the left side dull all over, from the clavicle to the costal cartilages; the rubbing, friction sound was also perceptible; the respiration was altogether tubular at the right side, there was besides dulness on percussion on the infra, mammary and dorsal regions; confused sonorous and sibilant râles being heard also. There was but little expectoration, following a hard painful cough; the former was glairy and adhesive. His decubitus was sometimes on the right side with his shoulders raised, turning occasionally on his back, and for short periods on the affected side. After being made more comfortable, and suitable remedies administered, he appeared to have rallied; and on the following day his countenance became clearer, and

the other symptoms seemingly mitigated; the dulness of the side affected was still impenetrable; the friction sound was scarcely heard, but œgophony was evident in the infrascapular regions; the sputa was not increased in quantity, but was quite characteristic; the breathing was still difficult and abdominal, but the immobility of the left intercostals was more apparent now, as he was less restless. On the approach of evening, after a disturbed sleep, the dangerous symptoms of dyspnœa reappeared; his face and extremities becoming livid, and his breathing hurried and gasping. It was not without much difficulty that I succeeded in restoring him from this condition. He continued much easier, without any remarkable remission of the morbid symptoms, until the fourth day, when he had another very alarming return of dyspnœa, with lividity of countenance, failure of pulse, jactitation, and delirium. He was again relieved from this state, and lingered on for three days longer, with frequent returns of those dangerous attacks, generally following sleep, from which he started in delirious terror, gasping like one suffocating, sometimes rallying, speaking rationally, taking nourishment, and the pains and active symptoms again returning with severity, when at last he sank gradually, without much suffering.

I had not an opportunity of learning the early medical history of this man's case correctly, and could only collect from himself, or some of his comrades, some imperfect particulars.—That, some five years before he had, while on his march through the interior of Ireland, a severe attack of inflammation of his lungs, and had to be left behind in charge of a private practitioner; that he had a relapse of the same, and was near dying of it subsequently; he was also in hospital on three or four occasions more in other places, but that for the last three years he was able to perform all his regimental duties remarkably well, attending to horses, &c.; that he was very short-winded, and not equal to violent exertions; he drank occasionally, when he could get it; and the evening before the last fatal illness he became very tipsy in a public-house, and amused himself by singing and dancing; it appears also that he felt himself ill and uncomfortable some days before this, but did not come to hospital, thinking that it would wear away.

*Post-mortem Examination.*—The body was well-formed and muscular; there was no remarkable difference apparent between the size of the two sides of the thorax, nor other external signs of chronic disease. On dividing the costal cartilages of left side, serous fluid came welling over, and I found the cavity entirely filled to overflowing with sero-purulent effusion, shreds of lymph being interspersed, and granulations of organised lymph studding thickly the costal sides of the cavity in various patches; the lung, much collapsed, and intensely carnified, was pushed over towards the mediastinum, and protruded into the right cavity of the thorax, and was attached in various parts by numerous bands of false membrane to the costal pleura, particularly at the superior part of lung in the subclavian region, interlacing and forming there a kind of cellular structure;

the heart and large vessels were not much displaced from their natural position; the former was large, but healthy; there was much congestion of the right lung, particularly at the base and posterior portions of the lobes, which were of a dark-brown colour; the liver was much enlarged and congested also.

This case furnishes a remarkable instance of the possibility of a person existing for so long a period without any extraordinary discomfort, although he carried within his thorax a copious effusion of fluid, having a collapsed lung, of little use for the purposes of life, with other morbid lesions attendant on this state. Still he ate, drank, and enjoyed himself like other men, and looked well and in good condition, and attended regularly to all his regimental duties. However, when an acute attack of illness occurred, brought on by his own imprudence, he readily succumbed, his chances being lessened by the consequent congestion of his right lung, there remaining but a small portion sufficiently sound to carry on the functions of breathing.

## February 13.

### Professor O'Connor in the Chair.

*Case of Non-Maculated Fever, terminating fatally, with Diphtheritic Symptoms.*—By Dr. Cummins.

On the 30th of April, 1858, when ague was very prevalent in the neighbourhood, I was called to attend Mrs. I, a full plethoric woman of forty-five, who was suffering from most intense head-ache and high continued fever, alternating with occasional diaphoresis. She had not closed an eye for several nights.

I immediately prescribed a draught containing tartar-emetic and opium, which produced copious diaphoresis and some sleep, with great relief of the head-ache, and other symptoms. After a few days, regular tertian ague became developed, which rapidly yielded to quinia, and she was restored to perfect health. On the 28th of May, 1860, I was again called to her, and found her suffering from the same symptoms, the head-ache especially being much complained of, while the fever, with pulse of 80, continued uninterruptedly; there was a partial exacerbation every second day.

I treated her at first with diaphoretics, and a few leeches behind the ears, cold to the head, &c., without relief.

I then tried small doses of quinia, which rather increased the head-ache, without doing good otherwise. She was sponged twice daily with tepid vinegar and water, and she got an occasional opiate with tartar emetic or James' powder, without which she never got sleep. Tongue continued clean, and pulse 80.

On the seventeenth day she became delirious when left to herself, but perfectly rational when roused.

On the nineteenth day there was an increase of the delirium, with tympanitis, and the pulse had risen to 100, the tongue con-

tinuing as before, *perfectly natural*. A draught of oil and turpentine somewhat relieved the tympanitis.

Twentieth day.—She seemed rather better, but the head-ache was much complained of. I then had the head shaved, and gave her six oz. of wine in the twenty-four hours.

Twenty-first day.—There was a tendency to coma, with some stertor when left to herself. The urine had to be drawn off with a catheter twice a day, the secretion being perfectly natural in quantity and quality. She can be roused, but soon lapses again into a drowsy state. No cough or dyspnœa, but two or three expectorations were tinged with blood. Blister to back of neck and head. Wine 12oz.

Twenty-second day.—Still in the same drowsy state, but hands are continually in motion, picking the bed-clothes, and there is constant muttering delirium; still can be roused to perfect consciousness. Pulse 90, natural in force, and fulness. The secretions from bowels and kidneys are perfectly natural, and the tongue exhibits the most perfect conditions of health. No return of bloody expectoration, but there is slight tympanitis. Dr. W. Townsend saw her with me to-day, and coincided in my most unfavourable prognosis.

On the 23d day I was very unwell myself, with feverish symptoms, and was not able to visit her in the morning. Dr. Townsend, however, reported to me that the tympanitis was very great, but that in other respects she continued in much the same state.

We agreed to give her three drachms of castor oil, with a drachm and a half of turpentine in a two-ounce draught. This she took about $11\frac{1}{2}$ A.M., and, immediately after swallowing it, complained of its burning her throat. At noon the throat was much complained of, and the symptoms were described to me as so urgent that I got up and went to her. I found her sitting up in bed, supported by pillows, perfectly rational, with a thready pulse of 86, suffering from most intense dyspnœa, and a large secretion of mucus, mixed with small patches of a white exudation, rattling in her throat, and flowing continually from her mouth.

The dyspnœa was so urgent that I found it impossible to make a satisfactory examination of her throat, more especially as vision was obscured by the copious secretion.

Intellect was perfectly clear; the pulse never rose above 86, and she was almost quite unable to swallow.

At six o'clock, P.M., on attempting to swallow some brandy-and-water, she became asphyxiated, and died in a few moments.

My treatment during the six hours which this last severe attack continued was directed to endeavouring to administer stimulants, and applying warmth to extremities, cardia, &c., and counter-irritants to throat externally.

I was so very ill myself at the time, and for several days after, that I was unable to make any post-mortem examination of the throat; but I find it difficult to account for the sudden invasion of the fatal symptoms and the rapid course they ran, except on the

supposition that diphtheria was their cause; and the antecedent typhoid debility, which took away from the patient all chance of wrestling with the fatal grasp of so formidable a complication, the reason why it ran so rapid a course.

It is worthy of note, that two days before there had been some sanguineous expectoration, which probably came from the throat, as there was no cough that would account for it otherwise. Did the throat symptoms commence then, and remain in abeyance until the draught of turpentine and oil, acting as a local irritant, called it into more energetic action? I think such must have been the case, notwithstanding the entire absence of other symptoms referred to that part during the intervening period.

This case recalled to my memory a clinical lecture delivered by Dr. Corrigan last February, and I have been fortunate enough to find it in the sixth volume of the Dublin Hospital Gazette. He takes as his text a case of non-maculated typhus, which occurred in the Hardwicke Fever Hospital, and which, on the sixteenth day, became complicated with diphtheria, which ran a fatal course in about twenty-four hours.

Dr. C., remarking on it in his usual lucid style, makes the following observations, which, I think, apply equally to the case before us:—

"Was the fever under which Mrs. I. laboured for several days one peculiar to diphtheria, or was it merely one of non-maculated typhus—diphtheria terminating life as an accompaniment or sequel—as we frequently see in other instances when local disease is prevalent, having a tendency to be developed towards the termination of any of our depressing fevers? The latter was probably the case in the instance before us; but this cannot diminish our fear about the disease—whether as an idiopathic disease, or as a sequel of fever, there is much to be dreaded about it."

This was one of the most treacherous cases I ever witnessed, and as such is remarkably interesting; the symptoms setting in of somewhat an aguish character, the pulse being little increased in frequency, the tongue and all the secretions continuing natural throughout its entire course, while at the same time the lesion of the cerebro-spinal nervous system became more and more profound from day to day, until near the fatal termination, the case presenting a group of symptoms contrasting strangely with each other.

Drs. Graves and Stokes long ago pointed out that unusual symptoms in typhus, even when favourable in themselves, should be regarded with suspicion, and this case most remarkably demonstrates the correctness of their observations.

*Pneumonia—its Treatment.* By T. W. Belcher, M. A. and M. B., Oxon. and Dublin, L. K. and Q. Coll. Phys., Ireland.

The object of the details in this paper may be briefly stated as an attempt to determine the best method of treatment in certain acute diseases, of which pneumonia is selected as an example.

During the half-year ending December 31, 1860, fifteen cases of this disease were treated in Dr. W. C. Townsend's division of the Cork Union Hospital. Of these nine have been discharged cured, five remain as convalescents, and *one* died.

The fatal case was that of a person, who, having for many years laboured under complicated disease of the respiratory organs, contracted pneumonia in a state of weakness and prostration, so that his chance of recovery was small. At various times, however, he rallied, and the disease in a measure subsided; eventually he died, and his case, which was carefully noted by Dr. Bernard, was, with the result of a post-mortem examination, exhibiting fetid abscess of the lung, read by that gentleman before this society at an early period in the present session.

If this case be removed from the category of "cases of pneumonia"—as the writer considers it ought—it becomes evident that the remaining fourteen all resulted in cure.

As the object of this paper is merely to determine the best mode of treatment, and not to give a complete history, *three* cases, noted in *extenso* by Dr. W. C. Townsend, shall, for the present purpose, be exhibited in tabular synopsis.—[*See Table on the next page.*]

To show, *in detail*, the application of the treatment adopted, one of many may be quoted:—

Daniel M'Carthy, aged 37, married, a labourer, of temperate habits, admitted on the evening of December 4th, a fornight after his discharge from the fever hospital, where he had erysipelas of the face.

December 5th. Complains of headache and cough, with pain in the chest; tongue white, red at the edges; skin hot and dry; pulse 92, and weak; respiration 34 in a minute.

On examination, the left lung was found *generally* solid, the excepted portion being very small: bronchial respiration was distinctly heard all over the solidified part; crepitus in the remainder.

*Treatment.*—Ordered 5 grains of Dover's powder every four hours; 8 ounces of wine; a blister to back and front of left lung; also beef tea and diluents.

6th. He spent a sleepless night; the blisters rose well, and he feels better; pulse 120, and weak; respiration 42; expectoration copious, and mucous; dyspnœa greatly relieved.

A little air passes through the upper part of the left lung.

Urine full of chlorides.

*Treatment.*— Continue Dover's powder and wine.

7th. Slept well; expectoration slightly rusty; pulse 140; respiration 32; crepitus continues.

*Treatment.*—Continue as yesterday.

8th. Pulse 120; respiration 30; other symptoms same as yesterday.

*Treatment.*—Continue powders and wine.

9th. Same as yesterday.

| Name, age, and calling of patient. | State of general health previous to admission. | Sober or intemperate in habit. | Portion of lung or lungs affected. | In what stage at admission. | Complications with other diseases. | State of urine. | Symptoms noted in each case. | Physical signs noted in each case. | Treatment. | Results. | Number of days in hospital. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| 1.—Daniel O'Connell, aged 40, farm laborer. | Good. | Probably sober. | Middle and inferior posterior part of right lung, and afterwards upper part of left. | Solidified. | None. | Chlorides absent. | Pain in chest; cough; expectoration; pulse respiration; skin; tongue; general appearance. | Various results of auscultation and percussion, particularly, puerile respiration; crepitus; bronchialbreathing, mucous ronchus. | Blisters to parts affected, Dover's powder, and mercury with chalk, of each, 5 grains every 4 hours (on first day); afterwards, omit mercurial, but continue Dover's powder for several following days, until near the close, when 5 grs. were given every *eight* hours; beef-tea and arrow-root; wine, at first, 4 ounces, increased to 6 ounces in the 24 hours, then decreased to 2 ounces, all given in small quantities. | Cured. | 13 |
| 2.—Michael M'Carthy, aged 40, labourer. | Good. | No report. | Whole of right lung posteriorly. | Lowest part solid, middle and uppermost engorged. | Recovered from an injury of right side shortly before admission. | Chlorides present, but in small quantity. | Ditto. | Ditto. | Dover's powder, 5 grains, with nitrate of potash, 10 grains every four hours, blister to part affected; beef-tea; wine 4 ounces, increased to 6 ounces, and afterwards reduced to 4 ounces. | Cured. | 15 |
| 3.—Patrick Noonan, aged 18, labourer. | Good. | Sober. | Middle and lowest lobes of right lung posteriorly. | Solidified. | None. | Chlorides present and lithates. | Ditto. | Ditto. | 12 leeches to part affected, and cupping afterwards; Dover's powder, 5 grs. every 4 hours; blisters; diluents; beef-tea; arrow-root; 8 ounces of wine, reduced to 6 oz., then to 4 ounces. | Cured. | 13 |

10th. Slept tolerably well; pulse 116; respiration 28; sputa viscid, and slightly tinged with blood; tongue cleaning; sounds clearer all over the chest.

*Treatment.*—Continue as before.

11th. Pulse 112; respiration 28; sputa improved, as also the pulmonic sounds.

*Treatment.*—Reduce 8 ounces of wine to 6 ounces; and Dover's powder from one every four to one every *eight* hours.

12th. Much better in every respect.

*Result.*—In a very few days this man became convalescent; the wine and Dover's powder being diminished as he daily improved; and in a comparatively short time he regained his health, without any apparent diminution in his ordinary strength.

It is quite obvious that the treatment generally pursued in these cases has been most successful as to the result; while it is also manifest that—with just sufficient exception to prove the rule—blood-letting, tartar-emetic in large quantities, and mercury—the sheet-anchors of orthodox antiphlogistic treatment—have been discarded; and not only so, but, as was formerly supposed its mortal foe, alcohol has, with good effect, been to some extent substituted for them.

The remedies used were intended to promote the free action of the skin, kidneys, and, to a lesser extent, that of the intestinal mucous membrane. Constant local stimulation was employed, while the powers of life were upheld by suitable nourishment, easy of assimilation, and calculated to compensate for the loss of muscular and nervous tissue occasioned by the wasting character of the inflammatory process[a].

All this proceeds on the presumption, that instead of the inflammation being attended with undue exaltation of the vital forces, it is a disease of nutrition, depressing the vital powers, and hence requiring support; and, though it doubtless appears plausible to assert that the want of appetite, weakness, and wasting, occurring in this diesease, are natural processes, forming "a set-off" against the undue exaltation of the vital forces, yet the more natural explanation of these phenomena is, that nutrition being arrested, these results plainly follow; and the obvious tendency of the treatment should be to uphold the vital powers by such food as can be easily assimilated. In this category alcoholic *food* stands; and medical men should not countenance the very prevalent opinion, that as a mere stimulant, and in that light only, should this important therapeutical agent be regarded.

On the principles above stated, it is manifest that the ancient and orthodox practice of general bleeding is inadmissible; and the old statement—that inflammation can be cut short by it—is, in the opinion of many practical men, a statement incapable of proof[b].

[a] See Todd's Clinical Lectures, p. 230, Beale's Edition, 1861.

[b] J.H. Bennett's Clinical Medicine, Third Edition, p. 282.

The use of mercury was discarded, because its vaunted advantages, in cases such as these, rest on mere theorising; while, in many instances of recovery under this part of the antiphlogistic system, it may be truly said the cure was worse than the disease.

Moreover, in the instances already enumerated, the patients recovered without it; so that, at best, it was unnecessary.

In the face of such evidence, is it not illogical and unfit that medical men should argue against a modern *practice*, because it goes to upset an old and time-honoured theory; and, while they recognise the importance of the pathological and physiological investigations of late years, yet steadily refuse to admit it in practice, thus making the discreditable avowal that medical knowledge and practice cannot agree?

One valid argument exists, that of experience. Let any one treat similar cases to those above-mentioned in a similar manner, and then declare the result.

Lord Bacon says,[a] "Every medicine (remedy) is an innovation;" and he adds, "It is true that what is settled by custom, though it be not good yet at least it is fit; and those things which have long gone together are, as it were, confederate within themselves; whereas, new things piece not so well; but, though they help by their utility, yet they trouble by their inconformity; besides, they are like strangers, more admired and less favoured."

Professor Hughes Bennett[b] enters largely into the statistics of various modes of treatment pursued in pneumonia; the following is a short summary of his statements:—

Result of vigorous antiphlogistic treatment, mortality, 1 in 3. Treatment by tartar-emetic, according to Rasori, and more recently, Dietl, 1 in 5; according to Lænnec, 1 in 10. Result of moderate bleedings, according to Grisolle, 1 in $6\frac{1}{2}$. Result of dietetic treatment, with occasional bleedings and emetics in severe cases, Skoda, 1 in 7; Dietl, 1 in 13. Result of treatment directed to forward the natural progress of the disease, J. Hughes Bennett, 1 in 26.

There can be little doubt of the great evils attendant on overstimulation in such cases as those now referred to; but practical experience has shown, that while under the antiphlogistic treatment, 1 case in 3 died; under treatment by large doses of tartar-emetic, 1 in 5 died; the result of more moderate bleedings was 1 death in 7; but a course of treatment directed to further the natural progress of the disease gives but 1 death in 26.[c]

The cases treated in the Union Hospital were those of persons of various ages, and in almost every variety of humble life. The average age was 41 years, the lowest 9, and the highest 66; thus showing that the plan adopted is suited to every condition of the disease.

The presence or absence of chlorides in the urine, coincidently

[a] Essay on Innovations.
[b] Clinical Medicine, pp. 289, &c.
[c] J. Hughes Bennett, *op. cit.*, p. 682.

with an opposite state in the diseased sputa, is insisted on by modern writers as a symptom of pneumonia of much importance, and only requires statistical observation to make it very valuable as a source of information.

The undoubted short period of convalescence in the cases before us is also deserving of consideration.

If, indeed, as has been asserted, the old theory of inflammation is incompatible with our present state of scientific knowledge, and if consequently the practice founded on such a theory be unsound, should we not adopt a practice which commends itself to all moderate men as simple in the using, successful in the result, and tending strongly to follow out the old Roman maxim—

"Salus populi suprema lex."

Lest it should be imagined that the treatment pursued in the cases detailed above is inapplicable to the better classes, and suitable only to the lowest conditioned of our population, I venture to append the following observations of Dr. W. C. Townsend, made during the discussion on the previous paper. These remarks serve also to elicit the views of one well calculated to form a practical opinion on the subject, of one who is a practitioner of long standing, and for several years an hospital physician in this city. To his kindness I am indebted for the suggestion that these cases should be brought before the profession, and for the ample opportunities afforded me for doing so. "For the last two years I have treated in private, as well as in hospital practice, a large number of patients labouring under pneumonia: in these cases I have not used a particle of calomel or any other mercurial, except as a purgative, and I have rarely, if ever, lost a patient."

With reference to the cases detailed in the paper now under discussion, I would not advocate an extreme principle of overstimulation, which, as far as my experience goes, I think quite unnecessary; but I would submit for the consideration of the profession, whether the old system of large bleedings, mercurialization, and keeping patients in the lowest possible state, may not have been carried too far; whether a different and more moderate course might not be adopted with great advantage; such course having in view the supporting the system, while the disease was being eliminated by the natural processes?

If we are to consider what is called "idiopathic pneumonia" as the result of a morbid process, disturbing the circulation, arresting nutrition, and necessarily lowering the powers of life, it may be well to ask ourselves whether a plan of treatment which, on the principle of subduing inflammation, has for its aim and end a further lowering of the system, may not be mischievous? Whether the depression of the vital powers, thus induced, must not be injurious, and very frequently terminate in a tedious convalescence, leaving the patient an easy prey to some other morbid tendency

till then lying dormant, but which, as long as the constitutional powers were strong, would never be called into action.

I very much fear that we are too apt to adhere to long established opinions, ideas, and systems of treatment. They, as it were, grow with our growth, and strengthen with our strength; the slightest deviation from them is considered an innovation. Would it not at least be worthy of consideration whether we do not trust over-much to therapeutical agents in controlling disease, and too little to the natural powers of the system?

I must confess my experience induces me to adopt the views of those who hold that more advantage is obtainable from assisting and controlling the natural processes, than can be obtained from adopting those extreme measures which have in view the presumed possibility of cutting short an inflammation or extinguishing a fever.

There is no denying the fact that our views are every day, insensibly to ourselves, changing; no sane person now either bleeds or mercurializes to the same extent in pneumonia, as was the common practice twenty years ago; and no matter how strongly the moderate plan of treatment, by which I mean moderate support, and the careful use of stimulants so as to uphold the system, while the disease is being eliminated, may be abused, it is now very generally adopted even by those very persons who decry it.

## February 27.

### Professor O'Connor in the Chair.

Dr. William P. Bernard related the following case of *Double Pneumonia, treated by Stimulants.*

Robert Williams, aged thirty-eight, a plasterer of intemperate habits, was admitted into the Union Hospital, under the care of Dr. Townsend, on the 21st of January, 1861, suffering from frequent rigors, accompanied by dyspnœa and pain in the chest.

Though emaciated and very debilitated, he states he never suffered from ill health until three days before his admission, when, early in the day (and while at his work), he felt a dizziness in his head, followed in the evening by a very severe rigor, which lasted from three to four hours, and again attacked him, but with less severity, on the following morning.

January 23, 1, p.m.—Is just recovering from a rigor, which the nurse describes very severe; his teeth chattering and his whole body trembling during the paroxysm; skin moist; tongue very hot, dry, tremulous, and covered with a brown crust in the centre, the edges, and tip red; bowels constipated; urine scanty and high-coloured; pulse and respiration very rapid; no head-ache; complains very much of pain in both sides of chest, from the nipple downwards; this and the dyspnœa prevent his remaining in one position for many minutes at a time, and he chiefly lies on his back; troublesome

cough; sputa scanty, rusty, and tenacious; marked dulness on percussion over root and base of both lungs posteriorly; bronchial breathing and broncophony both very well marked. These symptoms, though present over base of lungs anteriorly, are very much modified in degree.

Treatment. Bowels to be relieved by house medicine, after which five grains of compound ipecacuan powder, to be taken every four hours; to have eight oz. of wine until next visit.

January 24. Bowels freely relieved since last report; cough troublesome during the night, which prevented his getting sleep; tongue moist, but still hot, and very coated; pulse 100, regular and feeble; feels freer from pain, but no relief from the dyspnœa; expectoration more profuse; rusty, with blood-streaks on the surface; very tenacious, adhering firmly to the bottom and sides of the vessel.

To have turpentine stupes over base of both lungs posteriorly, after which large blisters to be applied. Continue treatment, giving him, in addition, half a pint of beef-tea.

January 25. Feels easier since the blisters have been dressed. Continue treatment.

January 26th. Slept better last night; tongue still very furred; pulse 100, but more feeble than hitherto; expectoration very rusty and puriform, but less tenacious, and not streaked; moist crepitation over anterior and inferior portion of both lungs, but more aggravated at right side; increased resonance over base of both lungs posteriorly.

Continue powders; wine to be increased to 12oz., and beef-tea to one pint. Tested the urine for chlorides, and found them largely present.

27th. But little change since last report.

28th. Slept soundly; pulse 88, extremely feeble; tongue cleaning; expectoration more tenacious, and freer from tinge and pus; moist crepitation over anterior inferior portion of right lung; respiration clearer over corresponding portion of left lung; moist crepitation over base and inferior portion of both lungs posteriorly. Continue treatment.

30th. Much improved since last report; wine to be reduced to 10oz., and the powder to be given every eight hours.

31st. Feels very much better; sleeps well; tongue cool, clean, and moist; skin soft; pulse 60, full and regular; expectoration copious and purulent, as if from an abscess; respiration very much improved. Continue beef-tea; wine reduced to 8oz., and the Dover's powder be taken only twice daily.

February 1st. Continues to improve.

2nd. Feels as easy and almost as well as before his illness; is anxious for a chop; wine stopped, and to have 6oz. of meat and a pint of porter daily, with bread *ad libitum;* his powders to be continued only at night. From this period he continued to progress rapidly, and at present enjoys good health.

Perhaps the chief point of interest in the foregoing case is the treatment adopted, and which has proved so successful in this instance; which is but one of many similar cases so treated in the hospital with a like result. One of the agreeable features in it is the rapid convalescence, which could hardly be expected from the severity of the case, and its tolerably frequent reverses. It at least shows that the stimulant plan of treatment is worthy an impartial and complete investigation. Who more suited to conduct such a research than those connected with our hospitals, where so many opportunities of comparison and complete supervision are to be had, and which could not be carried out with anything approaching the same certainty in private practice.

Professor O'Connor read the following:—

*A Case of Monstrosity; deficiency of the Abdominal Walls, of the Cartilages of the Ribs, and a part of the Diaphragm; Hernia of the Heart.*—Mrs. O. S. had symptoms of miscarriage between the seventh and eighth month of pregnancy. Slight hemorrhage continued for two days at intervals, at the end of which time labour-pains set in, and she was speedily delivered. To my astonishment, I found the abdomen of the child attached to the mother by a large bag, resembling the membranes of the uterus. Feeling through this bag I found the funis enclosed within; and while in the act of tying it, the placenta came away. The bag, which appeared to be the peritoneum of the child, was attached to the placenta, and contained the funis, which was not more than four inches long; and accordingly the abdomen of the child must have lain against the placenta whilst in utero, and produced the deformity I am about to describe. The walls of the abdomen were entirely absent, and the abdominal organs were seen hanging loosely, not being bound down by any covering. The liver and spleen were particularly pendant. The lower part of the sternum and cartilages of the adjoining ribs were also deficient, and the heart was quite visible, lying on a line anterior to the walls of the chest. The child never made any effort at respiration, which would have been impossible from the state of the diaphragm, but the heart was pulsating with considerable force. Having observed it for a quarter of an hour, and seeing the pulsations become weaker, and knowing there was no possible chance of resuscitating the child, I opened the pericardium, when I found at once the action of the heart become stronger. It beat then and for some time twenty-four times in a minute. If it grew weak, I touched it with a little spirits of hartshorn, and it became at once more excited; touching it with the finger also made it anticipate its time of action. I placed my finger on the aorta to retard the blood in its passage from the heart, and there was double systole, as we find in valvular diseases. In looking at this strange phenomenon, I endeavoured to realize to myself what the convictions of a person free from preconceived ideas would be as to the motions of the heart. After systole the heart lay in

a state of apparent collapse, from which it suddenly rose up as if by a convulsive shock, the most apparent effect of which was a visible swelling up of the heart, caused of course by the contraction of the vertical diameter, and the passage of the wave of blood through the ventricle. Any one not having studied the subject would have thought that systole was the moment in which the heart was filled, not emptied. During systole I could not distinguish a difference between the auricular and ventricular contraction, still the eye was satisfied that all parts of the heart did not act at the very same time; but occasionally, when the action of the heart became very feeble, I saw as if a vermicular movement of the right auricle, which did not reach the ventricle. My impression also was that there was a slight filling of the ventricle during diastole; and indeed I cannot imagine why this should not be the case at all times, when we know there is a continuous stream of blood always arriving at the heart from the veins, and there is nothing in the auriculo-ventricular valves during diastole or rest to prevent the blood passing onward to the ventricle.

The lady was delivered about eight o'clock, and I continued to watch the motions of the heart till ten o'clock. I instructed the nurse to observe the time at which it would cease, which she reported to me next morning was at half-past ten, two hours and a half after delivery. I regret this rare opportunity was not presented to some one more competent than I was to derive positive knowledge on this singularly difficult subject of the sounds and movements of the heart.

## March 13, 1861.

### Dr. Tanner, President, in the Chair.

*Thoracic Aneurism; Dilatation and slight Hypertrophy of Heart.*—Dr. Finn[a] having expressed his acknowledgments to Dr. Golding, Assistant-Physician to the North Infirmary, for assistance rendered in the *post-mortem* examination, exhibited morbid specimens of the above, and communicated the following particulars:—

Constable Denis Phelan, aged 51, of stout stature and tall conformation, was admitted into the North Infirmary on the 8th February, 1861, labouring under cough and hoarseness, which symptoms were reported to have commenced about six weeks previous to his admission to hospital. He had enjoyed habitual good health until January, 1858, when he fell from a height of fifty feet, whilst endeavouring to extinguish a fire. On this occasion he received some injuries of the chest and abdomen, for which he had been for some time under surgical treatment in the North Infirmary, and subsequently in Steevens' Hospital, Dublin. Since that period his health had never been so good as previously; he was more susceptible to the influence of cold; and exercise, however moderate, occasioned a shortness of breathing, from which he never suffered at any former period. Symptoms on the occasion of his admission—

[a] In Dr. Finn's paper, at p. 234, for "nitrate," read "carbonate" of bismuth.

Pulse 54; tongue clean; regular habit of body; appetite good; slight cough and hoarseness. He experienced some difficulty of deglutition, which applied equally to liquids and solids, and referred the source of the difficulty to a point corresponding with the upper third of sternum. Physical signs—Slight prominence of the upper part of the sternum to the right of the mesial line; percussion-sound normal; respiration stridulous; but auscultation did not suggest the existence of an increased secretion from the air-passages. The sounds of the heart presented no abnormality in their character. In the course of the ascending aorta and arch, a loud double sound was heard, loudest at the prominence of sternum, above noticed; but at no point was either sound replaced by a murmur.

On the 1st March his expectoration was reported to have been slightly tinged with blood; he died quite suddenly at noon, having previously expectorated about two pints of blood.

*Autopsy* took place about twenty-four hours after death. A very large quantity of fatty matter was deposited in the anterior mediastinum. The pericardium contained about two ounces of serum. The heart was both dilated and hypertrophied; and its surface generally was invested by a layer of adipose tissue, which presented considerable density near the septum cordis. The muscular element of the heart was pale, and appeared to participate in the adipose metamorphosis. The valvular structures were free from disease. The aorta was dilated in the first and second parts of its course. An aneurism of about the size of an orange involved the right lateral and anterior aspects of the vessel (the latter to a moderate extent), the points against which the blood-wave impinges with its maximum force. It commenced about an inch below the innominata, and extended thence to near the origin of the left carotid. The interior of the artery presented a high degree of vascularity, and exposed a surface rendered extremely rough and irregular by the intrusion of atheromatous and calcareous deposit. The sac contained a large quantity of coagulum, which, though laminated, was not decolorized. The rupture took place at the distal end of the sac by a small opening into the central and anterior portion of the trachea. Had the patient survived for a short time, a second opening would have taken place into the contiguous portion of the œsophagus, which presented an advanced stage of ulceration. Both lungs were engorged with blood.

*Remarks.*—The diagnosis was facilitated in this instance by the dysphagia (already referred to) and the stridulous breathing; by the former more especially. The other symptoms were so obscure as scarcely to warrant an affirmative conclusion. The visual phenomena, noticed by Dr. Gairdner (Edinburgh), in connexion with thoracic aneurism, were not observed, though the centripetal tendency of the distal portion of the sac must have involved pressure more or less direct on the lower cervical ganglion. In two other cases of thoracic aneurism, at the same time under treatment in

hospital, no irregularity was observed in the character of the pupils, though examination had been frequently made for the purpose of ascertaining the fact.

*On Excision of Joints.* By J. E. Curtis, Jun., Surgeon, South Charitable Infirmary.

Mr. President and Gentlemen,—In all our large hospitals and workhouses we have every day presented to our notice formidable and intractable cases of joint disease where treatment has completely failed; and where the case arrives at that point, when the experience and judgment of the surgeon are tested in the highest degree, where the life of the patient, in all probability, rests on the course he may adopt,—whether he will deprive the patient of his limb, or still trust to treatment, and the effects of nature. Another choice is still open to him, whereby a useful limb may be saved, as well as the patient's life. I allude to excision of the joint.

In coming to a decision as to his course of proceeding, he will have to depend on his own experience, judgment, and the recorded experience of others, which records, if carefully made, will be of the greatest use to him on such a trying occasion; and thinking it the duty of every surgeon to add his mite to such a record makes me now, gentlemen, introduce the following case (successfully operated on by our President, Surgeon Tanner) to your notice, as well as to afford a subject for discussion at this meeting. Before reading the daily notes carefully taken by myself, I beg to preface this by a short history of the operation, and its statistics.

I believe the honour of originating the operation of excision of the knee-joint first lay with Mr. Henry Park, of Liverpool, in the year 1781; his patient was a sailor, and had suffered for ten years from disease of the knee; and at the end of twelve months after the operation, this man was so far recovered that he was able to proceed to sea, and make several voyages, and performed all the duties of a seaman.

From this time up to the year 1830, we have had reported eleven or twelve cases operated on by Mülder, Crampton, and Syme, and of these five were successful, one partially so, and five failed.

Mr. Syme, who, at that time carried great weight with the profession, wrote against the operation; it was abandoned for twenty years. Again, in the year 1850, we are indebted to Mr. Ferguson for the reintroduction of this operation, and he was followed by many others, more particularly by Surgeon Butcher, of Dublin (who has added much to conservative surgery), by recording a series of cases occurring between the month of July, 1850, and December, 1854, in which he mentions thirty-one cases, five only proving fatal; again, from the month of December, 1854, to December, 1856, he records fifty-one cases, with only nine deaths.

Now, gentlemen, as far as I can learn, the mortality in amputation of the thigh is about one in three; and if in an operation which has been so very frequently performed (I mean amputation of the thigh),

the mortality is so great, I think great praise is due to those men who have introduced so excellent an operation as excision, attended with so little mortality, and leaving a useful limb.

I will now read for you an account of the case performed on at the South Charitable Infirmary on the 26th of March, 1860.

John Ahern, aged 20 years, farm labourer, was admitted into hospital on the 22nd of February, 1860. As far as he can tell, his previous history was as follows:—That about five years previously he perceived a slight swelling in his left knee-joint, which gradually increased for six months. He can give no reason for the swelling, the joint (so far as he knows), not having received any injury; sinuses formed, and a thin whey-like fluid exuded from it; what little pain that was hitherto in the joint ceased when this fluid appeared; at this time he could do his daily work, and walk pretty well, and the joint was not stiff; the openings in a short time closed, but reappeared in twelve months, and sinuses again formed in four places; the stiffness increased after this attack; the sinuses never healed up to the present time; however, he was able to continue his farming work up to six months ago, and from that date he got gradually worse.

On admittance into hospital a careful examination was made: the left knee-joint was found to be enlarged and rounded, with a semi-elastic swelling occupying the interstice of the joint. The limb was semi-flexed; he allowed slight passive motion in the joint without complaining of much pain, but the joint daily became more painful and tender to the touch, and the slightest motion gave him the greatest pain; he had a strumous aspect, and appeared of a delicate constitution; the integuments covering the joint were of a white colour; there were four sinuses, two at either side of the joint; he has two brothers and two sisters, all of whom are healthy, as also his father and mother.

After being in hospital some time, his health gradually began to give way; Surgeon Tanner, under whose care the patient was, came to the conclusion that it was in every way a fit case for excision; and communicating the fact to his colleagues and many experienced surgeons, who were also of the same opinion, he determined to excise the joint, and operated on Monday the 26th of March, 1860, before a large assemblage of medical men.

The operation 10 A. M.—The patient being placed on the table, great difficulty was experienced in getting him under the effects of chloroform, although he evinced great desire that it should. The operation was commenced by a semi-circular incision from without inwards, running beneath the patella, carried through the ligamentum patellæ and soft structures at either side of it. The flap thus made, and containing the patella, was turned upwards and freed to some extent; the lateral ligaments were then divided, the leg forcibly flexed, the crucial ligament cut through, and the joint dislocated. The under surface of the patella was scraped, it showing some signs of disease, the tibia was forced beneath the condyles of the

femur, and little less than one inch of the femur sawn off, the tibia acting as a guard to the vessels and soft parts; then the tibia was brought forward on the femur, and sawn off to the extent of three-fourths of an inch. There was some little difficulty, and some force had to be used in getting the bones in apposition; the leg was straightened, the flap drawn forward and three stitches put in it; two small arteries were only required to be tied; very little blood lost during the operation, but a free oozing of blood continued for some time from the smaller vessels; the limb being bandaged, a strip of thick pasteboard placed on each side of the joint and secured, the limb was put into a box-splint. At this time the patient required a large amount of stimulants, which had but little effect. On examining the head of the bones, the cartilages were found to be destroyed on both bones, as also the under surface of patella.

7 P. M., same day.—In consequence of the extent of oozing, Dr. Tanner sent for; patient a little delirious, but which soon passed away. The apparatus removed, and joint exposed, bleeding ceased, but shortly reappeared freely; joint again exposed; much startings in the limb; gets an opiate every four hours; slept for an hour or two during the night, and suffered little pain.

Tuesday, 27th. Pulse 100; tongue clean; pain diminished; starting ceased; bandages removed, and a many-tailed one substituted; the opiate continued.

28th. Pulse 110; tongue foul; was ordered a mild purgative and diuretic.

29th. Pulse 90; tongue much cleaner; leg required a little extension, a slight bow appearing in it; ordered an anodyne at bed time.

30th. Complains of much pain just above the pubes; has a slight suppression of urine; passed none for twenty-four hours; ordered a diuretic; better towards evening.

31st. Much improved; medicines continued.

March 1st. Improving; complains of no pain over pubes; wound examined; two inches of union have taken place, the remainder looking healthy.

2nd. Same as yesterday; wound looking healthy.

3rd. Little suppuration since operation, in comparison to the extent of wound. No stimulants given since operation, but a generous diet with broth.

4th. He steadily improved, and as I should say very fast, up to the time he left hospital, which was on the 22nd of June, 1860, four months from the date of his admission, three from the operation.

When leaving hospital, the limb was straight, very little shorter than the other; he could walk very well; suffered no pain, and his general health was very much improved.

Before concluding, I beg to draw your attention to one of the steps of the operation as performed by Surgeon Tanner. I allude to the manner in which the heads of the bones were removed,

which I do not recollect to have been done by any other person, and which is a great improvement to the operation.

It is recommended by authors, and one in particular (Surgeon Butcher, of Dublin, who has thrown great light on this subject), to use a particular kind of saw invented by himself. The qualities in it, he says, are, that it protects the soft parts and vessels, and where it is required to cut the bone obliquely. Now, in the operation as done by Surgeon Tanner, he used the plain amputating saw; and by placing the bones one on the other the vessels and soft parts were protected; and owing to the strength of the blade, he was able to cut straight without the blade bending, which I think might possibly occur with Surgeon Butcher's saw.

When the patient left hospital he returned to his home in the west of the county of Cork.

Since the above was written I have great pleasure in stating that the patient is alive and doing very well, and does not suffer in the least.

## March 27.

### Dr. Baine in the Chair.

Dr. Sandham exhibited the leg of a child, eleven days old, which he successfully amputated on account of a gangrenous ulcer above the ankle. He states: On the 11th of September, 1860, I was sent for in a hurry to attend Mrs. Margaret M——, aged 27. I found her much exhausted from hemorrhage, which introduced her labour. On examination, as was to be expected, I discovered a placental presentation, and the fœtal presentation was also preternatural. I at once turned and delivered, without much difficulty; the secundines speedily followed, and the hemorrhage and urgent symptoms ceased immediately. When the nurse was washing the child, she discovered the phagedenic, or rather gangrenous ulcer you see in the preparation, situated above the internal malleolus of the right leg; the tarsal extremities of the tibia and fibula were dislocated from the astragalus, or rather their epiphyses were removed, and they projected out of the ulcer; the tendons were exposed, and a foul, offensively-smelling ulcer occupied the soft parts around. I ordered a yeast poultice, which was continued for ten days, and to nourish the child. Healthy inflammation set in, and natural amputation progressed. This encouraged me to propose to the parents its removal by amputation, and at the same time I assured them of the probability of the child's dying during the operation. On the eleventh day, the grandmother holding the child in her lap, and assisted by my son, I took off the limb below the knee by a flap amputation, in order to have as large a cushion as I could, knowing that, should the child survive, interstitial growth would still go on in the tibia and fibula. The child, very small, and a female, only winced during the tegumentary incision. She sucked sugar during

the rest of the operation. One vessel required a ligature. The operation was performed on Saturday, the 22nd of September; the stump healed kindly, without any untoward symptom, almost by the first intention; the child improved, took food freely, arrow-root and a little wine, and I discharged her well on the 4th of October, twelve days after the operation.

This woman received no injury during the period of gestation. She and the husband deny ever having had syphilis, or any other venereal complaint. Now, the question for discussion that arises in this case is of some importance, as I, for one, am taught by it to be very cautious in future in pronouncing such and analogous appearances on children after birth as positively secondary, tertiary, or quaternary syphilis. Well, then, the question arises—is the specimen of disease now before us, which commenced in utero some considerable time before delivery, the result of a syphilitic taint? and, if not, what is it, and how produced? As an argument in favour of its being syphilitic in its origin, the mother, who is very small, but hardy, had four children before this one; all came to the full time; two were still-born, and two survived birth but from two to four hours. On the other hand, the fact of the stump healing so very kindly, and the child improving, is, in my mind, very much against the idea of its being syphilitic—there was not another diseased speck on the child. I must candidly confess that the subject of the preparation before you is now a short time dead, but certainly from nothing as the result of the operation, or of a syphilitic nature. I infer, from the statement of the grandmother, that some lung affection carried her off.

An interesting discussion followed the reading of the case, and all were of opinion that the disease was not syphilitic.

Dr. Sandham here said—I was present at the trial of a young woman and her mother during the present assizes for infanticide, and Dr. Donovan, of Skibbereen, was medical witness for the prosecution. He stated that he had read in Dr. Collins' work on Midwifery that he (Dr. Collins) has often heard the cry or screech of an infant, not only during the passage of the head through the vagina, but even while absolutely within the uterus. This struck me at the time, and I am now anxious to get the opinion of all present with reference thereto. I have had some experience, but never heard a child cry or screech under the circumstance stated. None present ever heard such a cry, and all appeared to be of opinion that it was impossible for a child to cry so as to be heard, or cry at all, while the head was within the uterus, or without the uterus in the vagina.

*Subacute Inflammation of the Membranes and Substance of the Brain; Ramollisement.*—Dr. Finn exhibited a pathological specimen of the above, and detailed the particulars:—

Sub-constable John Doyle, aged 23, unmarried, of robust figure, was received into hospital on the 12th March, labouring under irritability of stomach and pain of head. His illness, which

commenced about eighteen months before, was referred to over-excitement consequent on active interference for the purpose of quelling a riot. On the evening of the day on which the riot occurred, he was guilty of some excess, having partaken rather freely of punch. On that night he fell out of bed, and on the following day complained of vertigo. This symptom having continued for some days, he was sent to hospital, where he was under treatment for six weeks. On leaving the hospital, he was able to resume his duties, which he continued to discharge to within a month before last admission. On being questioned, he complained of pain in the head and behind the ears, the pain increasing in severity towards evening and night, and interfering much with his rest. Pulse 88, small; costiveness; coated tongue; heat of skin; eyes prominent and staring, both pupils being much dilated; slight ptosis of the right eyelid, and the angle of the mouth at the same time was pendulous. It was also observed that the tongue, when protruded, deviated to left side. He suffered at irregular intervals from paroxysms of fever, attended with excessive secretion from the skin, and during which his consciousness was more or less disturbed, but never to such an extent as to preclude his replying coherently to questions addressed to him. Towards the close of life, his voice became gradually more inarticulate; he retained. however, to the last, control over the voluntary muscles. Death took place quite suddenly on the 26th March, as if from an apoplectic seizure.

*Autopsy.*—Congestion of the membranes of the brain, with sub-arachnoid effusion; generally increased vascularity of the brain; considerable amount of serous effusion in left ventricle, effusion in the right to a moderate extent; in the anterior part of the left middle lobe was discovered ramollissement to a moderate extent; in no other part was there observed any deviation from the normal consistence.

*Remarks on the treatment of Acute Disease.* By Dr. Cummins.—I have been requested to re-open the very instructive discussion on the subject of acute inflammation, to which Dr. Belcher's paper gave rise; and with that view, rather than because they possess any features of peculiar interest, I beg to lay a condensed note of the following cases before you :—

Mr. S., aged 24, tall, thin, and sallow, of a delicate strumous constitution, has had a cough since he was a boy, when he suffered from inflammation of the lungs.

I was called to him at 5 A. M., of the 2nd of October, 1860, and was informed that he had suffered from rigors, headach, weariness of back and limbs, great thirst and prostration; has not slept for two nights; pulse was very rapid; tongue coated behind, dry and red at tip and edges. I prescribed a foot-bath, and minute doses of tartar emetic, &c., &c. This treatment was continued during the day with some relief, and at bed-time he took aperient pills, and a

draught the following morning; up to this time there was no cough or pain in the chest.

October 3rd. Good night; headach gone; but there is a short troublesome cough; inability to lie on the left side, where there is also a dull aching pain; pulse 108, weak, intermittent and compressible; respirations 24; expectoration copious, viscid and rusty; tongue moist, and covered with yellow fur.

*Physical signs.*—Comparative dulness over base of left lung, without vocal resonance or bronchial breathing, fine crepitus much marked over entire base; dry and moist bronchial rales over both lungs anteriorly and posteriorly.

On account of the state of the pulse, it was not without great misgivings that I ordered him a quarter of a grain of tartar-emetic every three hours. I also ordered a blister to the left side.

I visited him at mid-day, and found that the pulse had become firmer and more regular under the influence of the medicine.

October 4th. Pulse 100, regular; respirations, 28; pain less; expectoration as before; tartarized antimony, half a grain every third hour; beef-tea.

5th. Pain gone; can lie a little on left side; pulse 108; respiration, 28; complete dulness of left base, with bronchial breathing and bronchophony. Another blister to side; tartarized antimony, one grain every third hour; beef-tea.

6th. Improving; continue medicine, take 4 grains of blue pill, and 1 grain of powdered opium at bed-time. The medicine has not once caused vomiting.

7th. Spent an excellent night, and is much better; pulse 94, regular and soft; respirations 24; continue medicine and pill as before.

8th. Pulse 76; respiration 22; sputa diminishing, and is now frothy, and free from coloration. There is still considerable dulness over base of left lung, with marked bronchial breathing, and bronchophony, some crepitus surrounding dull portion. Nose bled freely. Omit tartar-emetic. Take three grains and a half of blue pill with Dover's powder, every four hours. Appetite for the beef-tea and toast increasing.

9th. Pills caused some vomiting. Pulse 76; respiration 24. Continue pills. In the evening respirations were down to 20. Continue pills. Wine 3oz.

10th. Improving; but cough is rather troublesome. Wine 4oz. daily.

11th. There is still considerable dulness, with bronchial breathing and voice, but no surrounding crepitus. Nose bled a little again. Respiration 23; pulse 76. Gums slightly affected. Continue pills twice a day. Take a mixture of ipecacuanha, ammoniacum, &c., when cough is troublesome.

13th. Convalescing rapidly. Continue pills at bed-time, and mixture as before. Expectoration now muco-purulent.

15th. It is astonishing how rapidly the solid portion of lung is resolving, and that without crepitus redux; can lie now perfectly well on either side; expectoration is diminishing; pulse and respiration are natural; takes an egg for breakfast, and meat for dinner, with good appetite, to which I add porter to-day. Take one grain of quina three times a day.

24th. I examined the chest carefully to-day, and find the dulness, bronchial breathing, &c., quite gone; respiration natural everywhere; cough gone; the acute attack seems to have carried off with it the old chronic cough to which he was subject. (Note—This did not return again).

Master E., aged three, had a most severe attack of croup about six months ago in England, and narrowly escaped with his life.

At one, A.M., on the 23d of August, 1860, I was called to him, and informed that, without any premonitory symptoms, he was again attacked with croup last evening.

I found him gasping for breath, with the most marked and severe tracheal respiration, and cough; face almost livid; skin hot; pulse very quick and hard. It was one of the most severe cases of the disease I had ever witnessed.

I immediately had him put into a warm bath, had hot fomentations applied to his neck, and gave him one-eighth of a grain of tartar-emetic every five minutes until he had taken a grain; after which he vomited, cough became soft, respiration became comparatively free, skin soft, pulse less frequent, and I left him about three o'clock out of danger. I directed that the medicine should be continued every three hours, and next morning lengthened the intervals to four hours; at that time I also gave him three powders, with a grain of calomel in each, which he took during the day. In two days from the commencement he was perfectly well. I could bring forward many cases of pneumonia and croup treated in the same or a similar manner, with the same satisfactory result; and I cannot call to mind a single fatal case of pneumonia, except an *old* man, many years ago, of the name of Millar, and I entirely forget how he was treated. Now, when it is remembered that I have been a dispensary physician for the last seven years, and that pneumonia is rather a common disease among the poor, I find it difficult to account for the difference between my results and the statistics of "tartar-emetic treatment," as given by Dr. Bennet, from Rasori (viz., one death in four and a half cases), except on the principle that I rarely exceed one grain every three hours, and omit the medicine as soon as there is evidence of solidification to any great extent.

I quite agree with those gentlemen who believe that inflammation must run a definite course when once established, and that venesection or even excessive local depletion may so reduce the vital powers as to render them incapable of perfecting the organization of the exudation which is thrown out in the second stage, thereby causing tedious convalescence, or still worse, its death (gangrene);

but I do not wish to ignore altogether the first stage of acute diseases, and to make the essence of inflammation to consist in exudation, as some seem to me to do. I believe that there is a stage of increased vital activity, and that the higher this runs, the greater will be the exudation which must follow. I believe that this first stage is more or less under our control, and that we should moderate it by tartar-emetic, local depletion, &c., never forgetting that a time must come when the safety of the patient will depend upon an active circulation in the affected part. It is because I think the lowering effect of tartar-emetic is temporary, and ceases a few hours at most after its use is suspended, that I value it more highly than any means that directly remove blood from the system—blood which cannot be replaced except by nutrition, at a time when the assimilative powers are more or less in abeyance.

I have no doubt many cases of pneumonia would recover if left to nature entirely; because, important as the lung is, a portion of its tissue may be incapable of exercising its function for some time, without much evil resulting; but is this the case in croup? Can we in such a disease sit down patiently and watch nature organizing the exudation, and absorbing it after its transformation, and the excretory organs beautifully casting the effete matter from the system? No; because we know that the exudation here, by preventing the access of air to the lungs, would certainly destroy life before nature had time for her operations. Surely, the most ardent and devout worshipper at the shrine of nature would attempt to anticipate, or at least moderate, exudation in such a disease: and, if able to do it in this, why not attempt it also in other acute diseases? All Dr. Belcher's cases came under treatment in the second stage, when exudation had already solidified the lung; in such cases I have long given up the use of calomel and opium, as not only useless, but positively injurious; contenting myself with a mild alterative course of blue pill or hydrargyrum cum cretâ, and even that rarely pushed even to plight ptyalism, never to salivation. Dr. William Townsend's cases have, however, shown that even the mildest preparations of mercury are unnecessary in the treatment of pneumonia, and if unnecessary, they are certainly more or less injurious. I am determined to put a complete non-mercurial plan to the test; and if my cases turn out as well as his have done, I shall be fully satisfied.

I should remark that the case of pneumonia I have brought forward occurred in a peculiarly unfavourable subject, and that it was complicated with general bronchitis, as indicated by the sputa, as well as by the physical signs.

*A Case of Fracture of the Femur, treated with Butcher's Splint.* By Thomas Powell, L. R. C. S. I., Medical Attendant, Dispensary, Enniskeen, late Assistant-Surgeon, H. M's. Bombay Army; formerly Lecturer on Practical Anatomy in the Peter-street (now Ledwich) School of Medicine, and Resident Surgeon, Anglesey Lying-in-Hospital, Dublin.

On the 14th of August, 1860, I saw W. L., a well-made, muscular, dark-complexioned man, five feet eight inches high, and 37 years old. On the night before, when returning from a fair, he had fallen on the road, and found himself unable to rise; a neighbour, passing by soon after, put him in a cart, and brought him home. When I saw him, he was lying on the floor, apparently in great agony. There was one man pulling the foot, and another pressing the thigh between his open hands, which the patient said relieved him. On examination, I found the left leg $1\frac{1}{4}$ in. short, the foot everted, and the thigh flattened and thickened. Upon making extension, crepitus was easily obtained; and I determined the existence of fracture of the femur in its middle-third. I immediately prepared splint and bandages; and, after bringing the limb to its normal length, I applied the long splint in the manner practised and with the addition recommended by Mr. Butcher, of Mercer's Hospital. The man immediately expressed himself as much relieved; on measurement, the limbs were the same length; and the startings of the muscles, which had caused great pain, subsided. Before I left him, he expressed himself as free from pain, and the altered expression of his countenance testified to the fact.

August 15th. Free from pain. Passed the night well.

August 29th. The length of the limb is still retained; free from pain and irritation of the skin, Reapplied the retentive bandage to the foot, and changed the perineal band,

September 11th. Changed the perineal band.

October 1st. Removed the apparatus, and applied a starch bandage from the knee to the perineum. The limbs are the same length; the foot holds its proper direction; the knee is a little stiff.

October 8th. Removed the starch bandages; the man moves about on crutches; the knee still stiff.

October 17th. Motion freer in the knee; he goes about on one crutch most actively.

October 31st. He this day discarded the crutches; when going out of doors, he carries a stick, but moves about the house without any assistance.

This case serves to illustrate in a remarkable degree the advantages of the practice introduced by Mr. Butcher, occurring in a distant part of the country, where the daily attention of the medical attendant was impossible. By reason of its superior power of retention, and the physical ease and in consequence endurance which the patient obtained, it enabled the fragments to be kept in a state

of perfect coaptation; and the case, under most unfavourable circumstances, brought to a successful termination. The superiority of Butcher's splint has been proved in many hands; but as this case shows such admirable results after so rough a test as country practice is to retentive apparatus, I think it tends to strengthen the evidence in its favour and adoption.

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*Case of Poisoning with Camphor.* By Thomas Woods, M. D., Parsonstown.

Mrs. P——, housekeeper, aged about 30, on Easter Sunday morning, intending to take a wineglassful of camphor mixture with a little sal volatile, for headache, took instead, by mistake, a wineglassful of a solution of camphor in alcohol. There were $2\frac{3}{4}$ ounces of camphor in 8 ounces of the spirit; she, therefore, must have taken 330 grains of camphor, with about a teaspoonful of sal volatile. She instantly felt a burning sensation in the mouth and stomach, and called for water. When the messenger returned, she was insensible, and vomited about 6 ounces of "something like her breakfast," according to the servant's account.

I saw her in about fifteen minutes after the taking of the camphor. She was quite insensible, but not comatose. She was rolling her head from side to side, and was very uneasy; her skin was slightly raised in temperature, and her pulse was full, but not much quickened; her breathing was not hurried, and the pupil of the eye was natural. She could not be got to swallow anything, and in about twenty minutes she had an epileptic fit of great severity, which lasted for about five minutes. She became very livid, almost black, in the face, and foamed slightly from the mouth. I endeavoured, when the fit was over, to introduce the tube of the stomach-pump, but could not succeed, owing to the obstinate resistance unconsciously made by the patient whenever it was got into the mouth. Even this was a matter of much difficulty, from the tossing about of the head, and rigid closing of the teeth.

I took away twelve ounces of blood, and placed in the mouth tartar emetic and ipecacuanha wine in rather large doses, but with very little effect, as vomiting was hardly induced, and I saw no camphor in what was evacuated. I also had the bowels well cleared out with enemas of warm water, and cold water dashed on the face and neck. These means were all that were used; and she regained consciousness in about an hour, and gradually recovered, being in twelve hours free from all bad symptoms, complaining only of weakness and slight nausea.

The interest of this case lies, not so much in the quantity of camphor taken (although I believe it is the largest on record) because I do not know how much might have been thrown up before I saw her, but in the well-marked epileptic fit, which occurred in about half an hour after the dose was taken; and in the absence of all

symptoms of after-irritation of the stomach, or affection of the head or heart, although a sufficient quantity of the drug must have remained in the system to produce the immediate symptoms related. Since the above was written, I have heard that for about a week after the dose was taken the intellect was not quite so clear as usual, especially after sleep. The patient fancied every day was Sunday for half an hour after rising from bed. There was induced also an unusual torpidity of the bowels.

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*On the Therapeutic Influence of the Southern Climatic Sanatoria, particularly with reference to Chronic Tuberculosis of the Lungs.* By Dr. RULLMANN, of Wiesbaden.[a] Translated from a reprint from the *Würzburger Medicinische Zeitschrift,* 1861. Band 2. By WILLIAM DANIEL MOORE, M.D., T.C.D., M.R.I.A.; Honorary Member of the Swedish and Norwegian Medical Societies.

IT is an ancient medical practice to send chronic patients, and especially those affected with pulmonary disease, to seek for cure or amelioration in a southern climate, and it has also long been a public custom to travel for health's sake to the south; yet until lately this has been accomplished only by a few. But since Europe has been intersected in almost every direction by railways, since the iron road has surmounted the very Alps, since steamships traverse the Mediterranean Sea in regular postal routes, enabling patients without danger rapidly and easily to take an extensive journey towards the south, hundreds of invalids wander annually, on the approach of winter, from the northern regions of Europe, to look for recovery in a more southern climate. It is chiefly the coasts of the Mediterranean which are thus resorted to by patients. Gradually certain places have come into repute, which, on account of their favourable local and social circumstances, are regarded as the most suitable for invalid stations, and which are now designated as southern climatic sanatoria. Of these the following should be placed in the first rank: Malaga, Hyères, Nice, Venice, Pisa, Rome, Palermo, Cairo, Algiers, Madeira. These are the places at present most visited and recommended, and with which we are medically best acquainted. In the second series those may be enumerated whose importance as climatic sanatoria is as yet but little known, has only recently been suggested, or is by many altogether doubted; such are : Cannes, Villafranca, Mentone, San Remo, Naples, Messina, Catania. The climatic sanatoria of the Alpine regions, as Meran, Gries, can no longer be considered in like manner as southern sanatoria.

Though a southern climate has long been esteemed and used in

[a] Read in abstract at the Congress of Physicians of the Middle Rhine, at Frankfort, on the 13th of October, 1860.

medicine as a remedy, and although the southern climatic sanatoria have for many decennial periods been visited by a great number of patients, the state of medical knowledge as to these localities is still very defective, and special indications for the several places can scarcely be laid down. If we compare the knowledge we have with respect to them with that which we possess as to our medicinal springs and baths, we shall find that we know incomparably more and much more certainly about the latter, although even with respect to them medical indications are still very vague and undefined. While we have accurate physical and chemical investigations of the waters of our bathing places, we are unsupplied with the necessary accurate observations, continued through many years, of the meteorological conditions and other cosmical agents of our southern climatic sanatoria. With regard to baths, we find the results of much medical experience upon record. But we shall in vain seek in medical literature for frequent and accurate reports on the effects of a southern residence in various diseases. A well-founded scientific treatment of this subject has only most recently been commenced; and only quite lately have good monographs been published on some southern climatic sanatoria, as that of Mittermeier on Madeira (1855), that of Reil on Egypt (1859), that of Vivenot on Palermo (1860), that of Sigmund, &c. But much still remains to be done. These very writers complain loudly of the defective and uncertain climatology of these places and of the sparing medical material which still exists. Consequently, there prevails among medical men in general a great uncertainty in the appreciation and use of the southern sanatoria. Practitioners are often in doubt as to what disease is best adapted to a southern climate. On the one hand, too much is hoped for—on the other, too little is expected, from this change of climate. Indeed, the beneficial influence of a southern climate on the disease against which this has hitherto been most employed, namely, chronic pulmonary tuberculosis, has been altogether questioned. But, notwithstanding this uncertainty and diversity of opinion, resort to southern climatic sanatoria yearly increases; and a more accurate study thereof, and the establishment of definite indications, constantly become more important and more urgent. In the following pages I shall endeavour to fix the points of view on which the study of these localities must specially be based, and to propound the principles suitable for the estimation of the medical material at present accessible, and I should hope thereby to contribute something to the due appreciation of these sanatoria.

In my opinion it is advisable in the first instance to consider only those diseases in which a southern climate has hitherto been most recommended and employed remedially, and with respect to which, consequently, the largest amount of medical experience is on record. These are chronic bronchial catarrh and chronic pulmonary tuberculosis. Let us first inquire: is amelioration or cure to be expected in these two morbid conditions from a residence in the so-called southern climatic sanatoria, have we scientific grounds for such a

belief, and does sufficient and reliable experience exist upon the subject? By such a limitation of the question, we shall sooner and more easily arrive at a sure and practical result. By endeavouring to investigate the several climatic agents and their physiological effects, we shall certainly obtain scientific points of support for the estimation of the influence of a climate on certain diseases; but these effects are as yet in general but little known; and even if they were generally known, we could not thence alone deduce the medical importance of the southern climatic sanatoria, because our knowledge of the climatic circumstances of these places is as yet very defective; besides, the physiological effects of given remedies admit of only a limited conclusion as to their therapeutic value: finally, we have here to do with a co-operation, the result of the sum of a group of very different cosmical and other influences. We are therefore compelled, in investigating these sanatoria, to invoke also the aid of empirical experience; indeed, as the matter at present stands, medical empiricism must still serve as the principal ground of our decision.

In the estimation of climatic sanatoria, especially with reference to their employment in chronic pulmonary diseases, three climatic elements have at all times been taken into consideration, namely, the temperature, the degree of moisture, and the pressure of the atmosphere. Even if these do not constitute all the influences which affect a person labouring under pulmonary disease, who resorts to the south for the recovery of his health, and which have an important bearing upon his morbid condition, they possess the greatest importance in our inquiry. The general opinion is that a moist warm climate, with an uniform temperature, is the most suitable to patients labouring under chronic pulmonary disease, especially to such as are affected with tubercle. As to the importance of different atmospheric pressure, there is still certainly much diversity of opinion. Some will have their tubercular patients exposed to a slight atmospheric pressure, and they quote the high pressure of the southern climatic sanatoria to the prejudice of the latter. Others consider the climatic sanatoria on the shores of the Mediterranean Sea so suitable as a residence for such patients, precisely because the atmospheric pressure is there higher than in most regions of the northern inland parts of the Continent. Such generally propounded and exclusive propositions have at all times justly met with opposition, and have not much promoted our knowledge of the climatic sanatoria. It has, on the contrary, only impeded investigation as to the remedial efficacy of a climate, that people, leaning on some empirical observations and physiological hypotheses, have seized upon a single climatic element and considered this to be the only active or only important agent in the cure, supposing all other elements of climate and other local influences to be unessential, and unworthy of closer examination. I am of opinion that both physiological theory, and also and especially medical experience, are against the assumption of the exclusive importance and absolutely necessary existence of certain climatic factors in the cure of those diseases. Even if we must suppose that

the greater warmth of the southern winter is always favourable to our patients, who for chronic affections of the lungs visit the southern sanatoria, I do not believe that a great uniformity of temperature, or a high degree of hygrometric moisture, is absolutely necessary: indeed, in many cases the latter is not even desirable, and just as little does a particular amount of atmospheric pressure appear to me to be an essential condition to recovery. I rather believe that these meteorological elements may be present in different degrees and in various combinations, and yet—with the co-operation of various factors extrinsic to climate—may produce an equally favourable total effect.

All these climatic sanatoria possess in common a greater degree of warmth, and this is certainly to be regarded as an extremely important curative agent in chronic affections of the lung. That an elevated degree of atmospheric temperature exercises a beneficial influence on our pulmonary patients, and especially on those affected with pulmonary tubercle, is proved by the fact that these patients always feel better in our summer than in the cold seasons of the year—winter, the beginning of spring, and the end of autumn; as well as by the fact that individuals affected with chronic diseases of the lung, when transferred from the north to the south, recover and become comparatively well; while, on the other hand, those who migrate from the south to the north are very frequently and seriously visited with pulmonary diseases. Moreover, the warmth must not exceed a certain degree, if it is to work beneficially on our northern patients. In this respect I would call attention to the fact, that all southern climatic sanatoria, to the efficacy of which in chronic pulmonary diseases medical experience bears testimony, lie within the temperate zone. But the tropical zone, according to statistical investigations as to the disease and mortality of the French and English colonies, is extremely prejudicial to European immigrants who are predisposed to pulmonary tuberculosis, or may be in the early stage of that disease; and it is a well-ascertained fact that within the Tropics chronic tuberculosis of the lung very easily runs into acute and rapid pulmonary phthisis[a].

In Cayenne the French exiles die not merely of yellow fever, dysentery, and other southern diseases, but also in large numbers of pulmonary phthisis; and in Ceylon, Calcutta, and Madras, the same disease is said to find many victims among the English immigrants.

We cannot at all, or only under strict precautions, recommend to such patients a summer residence on the shores of the Mediterranean, even though remaining there during the winter may be highly advantageous to them. Indeed, we observe that these patients are worse even in our northern summer, when the latter is unusually hot; and it is, with reason, thought advisable to send such patients

[a] See J. Rochard, "De l'Influence de la Navigation, et des pays chauds sur la Marche de la Phthisie." Paris, 1856; "Gazette Hebdomadaire," 1856 and 1857.

during the hot period of our summer to cool localities—to mountains, to the sea coast, or to sylvan districts. How far an elevation of temperature may in those cases be advantageous will, perhaps, be best measured by the temperature of an ordinary summer.

Much importance has been attached to uniformity of temperature, and it is certain that great and sudden changes of temperature act injuriously on those affected with pulmonary disease; but a moderate alternation has, for the most part, a beneficially exciting and strengthening influence on them, particularly on those who are of weakly constitution, while a great uniformity may produce a prejudicial relaxation. We observe this often in our summer. When an elevated temperature has prevailed during the day, a mild night and morning coolness has a peculiarly animating and refreshing effect, both on the healthy and on invalids. Alterations of temperature have no longer an injurious influence, when they occur with a certain constancy and regularity. Thus, for example, in the Egyptian winter the variations of temperature between early morning and noon are not inconsiderable, but as they occur to-day, they return on the following day in a similar manner[a].

As to atmospheric moisture, a certain degree of it has by many been considered as an essential condition of the utility of a southern climatic sanatorium in chronic pulmonary affections. It is, in fact, not to be disputed that a moist warm atmosphere is very frequently of decided advantage in chronic diseases of the lungs, by reason of its loosening and generally composing effect; and southern climatic sanatoria with considerable atmospheric moisture, as, for example, Madeira, may exhibit numerous favourable results—indeed, it may be said that the southern climatic sanatoria hitherto most resorted to and esteemed lie on the seacoast, and must, therefore, present a certain moisture of the air. But, on the other hand, it is not to be denied that in many instances a dry, moderately warm air, by its secretion-diminishing and general vivifying effects, and by promoting nutrition, may have a favourable influence on the course of those diseases; and there are medical observations on record, according to which a relatively dry warm climate has afforded special service as a remedial means. That in the adoption and estimation of the several southern climatic sanatoria the high degree of moisture is no longer in practice considered to be the principal point, is proved by the fact that of all the climatic sanatoria of the French coast physicians now give the preference to the drier Hyères; in like manner Algiers, which, in comparison to the other places on the shores of the Mediterranean, presents a very slight degree of atmospheric moisture, is preferred by many physicians to all the Italian and French climatic sanatoria. Moreover, the air of Egypt, of the suitability of which as a winter residence for patients labouring under chronic affections

[a] See my Reports on the Climate of Egypt, "Deutsche Klinik," 1859, Monatsbl. 4; and "Archiv f. Phys. Heilkunde," 1859, iii. 389.

of the lungs we have the most positive proofs, is drier in winter than that of any other similar sanatorium; compare the meteorological observations of Coutellé, Destouches, Reyer, Uhle. Indeed, in Egypt patients are even recommended to make excursions to the desert, with its fine, moderately warm, but dry, air, and often to sojourn there; and it is in that country precisely the districts adjoining the desert, or easily accessible to its air, which are thought most suitable for permanent invalid residence. These are Cairo, to whose gates the desert extends, and the narrow valley of the Nile, above Cairo to Nubia, the so-called Upper Egypt, which is compressed between great desert plains[a]. But the damp Delta, and the still damper sea coast, with Alexandria, are avoided by invalids. When we send patients affected with pulmonary tubercle in summer to high mountains, we can certainly not reckon upon much moisture in the atmosphere, and yet many such invalids are very well there. I would also observe that in countries whose climate is notoriously to be classed among the dry, pulmonary tuberculosis rarely occurs, as in Egypt and in the steppes of Kirghiz[b]. All these statements suffice to show that of the several climatic elements which may influence the development and course of chronic pulmonary tuberculosis, dry air is not to be regarded as an absolutely unfavourable agent; but that, on the contrary, it may in many cases have a positively favourable effect.

The influence of the various degrees of moisture is, indeed, for the most part, exercised upon the lungs and the system at large. Thereupon there exist no accurate physiological experiments. But they possess, moreover, an indirect influence, inasmuch as the moisture modifies the temperature and its effect upon the organism. Thus it may be taken as a rule that the moister the climate is, the more uniform will be the temperature; but it is at the same time to be observed that the moister the air, so much the more will the cold be felt, particularly when the air is in motion, and so much the more penetrating will be the variations of temperature; while, when the air is dry, neither the cold nor the variations of temperature, which in this case are certainly more considerable, are similarly felt. By the humidity of the air I mean the amount of watery vapour appreciable by hygrometic measurements; and it is, there-

[a] It is an error to suppose that the patients frequenting the Nile, in Upper Egypt, are, by constantly remaining on ship-board, always exposed to a damp atmosphere, and that thus the beneficial influence of the climate upon them is to be explained. The air in Upper Egypt is to be considered dry, not merely on the shores of the Nile, but also on the river itself. Uhle, who made all his hygrometric observations on board-ship, states that "the air on the Nile between 24° and 22° of Northern latitude is one of the driest of those as yet examined." See Uhle, "Der Winter in Oberägypten."

[b] Compare the writings of Pruner, Griesinger, and Reyer on the Diseases of Egypt, and Dr. Neftel's "Beobachtungen aus den Kirgisen-Steppen. Würzburger Med. Zeitschrift," 1860.

fore, to be distinguished from showers and rain, which may likewise have an important influence upon disease. Thus the rarity and short duration of rain is to be considered as a favourable element, as the patient is thereby afforded the opportunity of frequently enjoying the open air, and taking suitable exercise. It is also to be observed that the humidity of the air, that is the amount of watery vapour contained in it, does not directly coincide with the frequency of rain. It is indeed true that with a dry climate a greater rarity of rain is connected; and also that, where rain frequently falls, a greater amount of moisture can be demonstrated in the air; but frequency of rain is not necessarily connected with a moist climate. Thus, for example, Venice has in winter, according to hygrometric measurements, the dampest air of all known southern climatic sanatoria, and still less rain falls there in the winter months than in any other Italian or French climatic sanatorium (except Nice), and less than in Madeira.

As to the influence of high and low atmospheric pressure upon the development and course of pulmonary tuberculosis, directly opposite opinions have been advanced. On the one hand it has been asserted that a high atmospheric pressure acts most favourably on the course of this disease, and on the other it has been stated that a low pressure is to be regarded as a favourable element in the treatment. In support of each opinion, physiological reasons and empirical proofs have been brought forward. On the one side it has been advanced that all southern climatic sanatoria, of which we possess favourable experience, lie on the flat sea coast, and are consequently exposed to a high degree of atmospheric pressure; that the physiological effects of even strongly compressed air, as in the occupation of diving and in the works connected with the building of new bridges, are in general well borne; that the physiological changes produced by high atmospheric pressure, as retardation of the respiration and circulation, driving back the blood from the peripheric parts, development of the so-called venosity, can act only advantageously on the course of those diseases; and that, finally, direct experiments have established the favourable influence of compressed air upon chronic pulmonary diseases, and especially upon chronic tuberculosis of the lungs[a]. On the other hand, it is stated that experience shows that many patients labouring under pulmonary tubercle are very well in high situations, and therefore under diminished atmospheric pressure, and that they have been frequently sent with advantage to lofty mountains (Righi, Gais); that, moreover, the physiological effects of diminished atmospheric pressure, as increase of appetite, improve-

[a] On the therapeutic employment of compressed air in the so-called baths of compressed air of Pravaz, Milliet, Devay, see "Canstatt's Jahresbericht," 1854, ii. 225, and "Gazette Hebdomadaire," 1859, 783. Further, an essay on the Influence of Altered Atmospheric Pressure upon the Human Organism, by Vivenot, "Virchow's Archiv," xix., 492.

ment of digestion and of blood-formation, acceleration of the circulation, and promotion of the metamorphosis of tissue[a], must be advantageous to patients with weakened constitutions; and, lastly, it is alleged that pulmonary tuberculosis diminishes in frequency with the elevation above the level of the sea[b]. But even if these physiological and empirical statements which have been brought forward on either side in support of the different views of the writers are in themselves correct, they can by no means prove the general and exclusive validity of any of these opinions. It is much more probable that, in these pulmonary diseases, there exist different conditions and stages, as well as diversities in the local affection and in the general health, according to which at one time a diminished, at another an augmented, atmospheric pressure will be advantageous. What the extent of this advantage is, or how great is the physiological influence of the varied atmospheric pressure, as it occurs at the ordinary elevations and depressions of the surface of the earth, is not to be decided by our present experience; but it is certain that in many cases the climatic influence, as well as agents extrinsic to climate, act so powerfully upon the organism, that the atmospheric pressure is not to be considered, its influence being masked or counteracted. Lastly, it may be mentioned that the effect of atmospheric pressure, even if it is considerable, does not depend solely on the absolute elevation, but also—apart from the morbid condition—on individuality and habit, as the action of the same atmospheric pressure must be different, accordingly as the patients have previously lived under another more or less different pressure[c].

For a climatic sanatorium, the purity of the air is also of great

[a] Dr. Brehmer, who is a zealous advocate of the opinion that lessened atmospheric pressure is the most important agent in the cure of pulmonary tuberculosis, includes, among its physiological effects, diminution of the determination of blood to the lungs, and he appeals in support of this view to experiments by Volkmann and Poiseuille? "Baln. Zeitung," viii. 289.

[b] [Dr. Mühry, for example, in his work on Climate, "Klimatologische Untersuchungen," &c., Leipzig und Heidelberg, 1858, attaches the greatest importance to the amount of atmospheric pressure to which phthisical patients are subjected. He states that "the occurrence of phthisis diminishes with the atmospheric pressure in vertical elevations;" and he suggests that sanatoria almost ensuring an immunity from phthisis, "might be found on the eastern side of the Andes, in Mexico, or in the Sierra of the Andes of South America." See British and Foreign Medico-Chirurgical Review, vol. xxiii., p. 62, where I have contributed an analysis of his work.—TRANSLATOR.]

[c] The physiological phenomena usually ascribed to changes of atmospheric pressure, may as well be attributed to other atmospheric changes present at the same time,—alterations in temperature, moisture, and current of air. Richerand says:—"Were it conceivable that—the temperature, the current of air, and the degree of hygrometric moisture remaining the same—there should be such a change in the atmospheric pressure as corresponds to the daily and yearly variations in the barometer, the effect of the latter upon the human system would be, not to say none, at all events so slight, that neither would it be subjectively felt even by the most sensitive persons, nor, *à fortiori*, would it be objectively demonstrable.—See Vivenot's Essay in "Virchow's Archiv" already referred to.

importance, and must certainly be reckoned among the favourable curative agents in pulmonary diseases. The air must be free from foreign organic and inorganic admixtures, especially from products of decomposition derived from the animal and vegetable kingdoms, from so-called miasmata, as well as also from irritating hard particles of dust. Now, we find the air particularly pure when it is frequently moved, when it blows over large, unbuilt upon, and not overgrown, surfaces, as well as in its higher layers, on which account a peculiarly pure air is possessed by those southern climatic sanatoria which are situated on isolated islands, as Funchal, in Madeira—on the sea coast, as the sanatoria on the shores of the Mediterranean Sea—or in the neighbourhood of the desert, as Cairo[a].

In a climatic sanatorium the winds come next under consideration, in so far as, from their origin and direction, as well as the intensity and frequency of their occurrence, they affect the temperature, the degree of moisture, and the purity of the air; and, under the influence of the winds, the action of these climatic factors on the system varies. Thus a moist and cool temperature acts much more intensely when the air is in motion than when it is at rest; on the other hand, winds moderate the effects of elevated and dry warmth. But strong winds may, in themselves, be important to those labouring under pulmonary complaints, as they mechanically impede and disturb the respiratory movements. In some places the winds require special attention, as their influence is so prominent as to give to a locality its climatic character, and thus to decide the suitability of such a sanatorium. Thus the maestral, a dreaded north-north-west wind, in the south of France, extends its effects to Nice, and renders almost the whole of the French litoral, except a few points protected by mountains, an unsuitable residence for pulmonary patients, especially those affected with tubercle, who are in an advanced stage of the disease. And those hot winds blowing from the interior of Africa, the Chamsin of Egypt, the Samum of Algiers, the Sirocco of Italy, which are particularly felt in the north of Africa, are said to determine patients, at the time of their occurrence, in April and May, to leave the south.

A climatic agent still remains to be mentioned, to which little value is usually attached, but the importance of which, to invalids, should not be underrated—that is, the clearness of the sky. "The degree of habitual clearness and brightness of the heavens," says Humboldt (Cosmos), "is important, not merely for the increased

[a] It is incorrect to suppose that the air of the desert or its vicinity must necessarily be full of dust. The air of the desert, surrounding the valley of the Nile, is almost always extremely pure and clear. The sand of the desert is raised only by violent storms, almost exclusively by the storms from the south, which occur periodically in May. The dust met with at other times in Egypt is derived from unpaved clayey roads and places in the towns, the roads, and causeways at the Nile. In the plain and on the river itself, as well as in the adjoining desert, there is never, or only extremely rarely, any dust.

warming of the ground, the organic development of plants, and the ripening of fruits, but also for the sensations and entire disposition of man." When we consider that a cloudy day depresses a healthy man, while, on the contrary, a clear sky excites and enlivens his spirits, how much more is this not the case with an invalid; and how important, in this instance, is the reflexion thereof upon the bodily condition. But an invalid who leaves his home on account of his health, and lives in a strange neighbourhood and under unusual circumstances, has a very peculiar need of such external stimulus. A blue sky and bright sunshine give an additional charm to nature, and excite the beholder to more attentive contemplation; but the contemplation of nature occupies, calms, and rejoices the patient. His thoughts are withdrawn from his own sufferings; he is animated to the enjoyment of the open air, and to bodily exercise. In this respect we must regard the greater clearness of the sky, which is peculiar to southern regions, as in itself a circumstance favourable to the cure of those diseases.

There are still other climatic influences to be considered, which, indeed, are not usually of equal importance, but which, under certain circumstances, may have a very important bearing on the course of diseases. Such influences spring from the altered local and social conditions in which the patient is placed, from his altered occupation and mode of life. It is, therefore, important to be aware of, and to take into account, the particulars of residence, food, water, convenience, social pleasures, the opportunity of active and passive exercise, such as walking, riding, driving, &c.; also the mental enjoyment offered by the locality. Thence we derive, not merely direct influences on the physical condition, but also equally important influences on the intellectual life of the patient. If it be true that lasting mental emotions and excessive intellectual labour may have an unfavourable influence on the development and course of the diseases mentioned, the removal of such injurious agents may also have a favourable effect. How many of those patients, who have visited such southern climatic sanatoria, have been indebted for their improvement and comparative cure, perhaps, chiefly to the circumstance that, by the journey and change of scene, they have been withdrawn from injurious mental influences or excessive intellectual exertions. However, the new circumstances do not always act favourably. If many patients are agreeably stimulated and occupied thereby, others become dispirited and depressed. It is, indeed, in general, a great advantage when the patient finds in his sanatorium entertainment and diversion—and whether this is the case depends on personal inclination, on the degree of education, on the professional occupations and other individual circumstances of the patient; but it is also possible that this entertainment and diversion may be injurious. For example, Rome is recommended as a climatic sanatorium, partly on account of its treasures of art, which may afford to the invalid agreeable intellectual occupation and diversion; but it must not be forgotten that patients may

easily forsake the proper object of their visit, and make the study of the treasures of art the first point. In that case this occupation leads only to an injurious excitement; and the churches and halls, in which these treasures are located, are then to the patient nothing but frigid retreats, wherein he exposes himself to cold. In like manner, the patient is agreeably and suitably occupied in Egypt in considering the glorious antiquities on the banks of the Nile, if he does not in the study subject himself to undue exertion—but how easily does not zeal grow with contemplation! he drives, rides, goes from place to place, and so gives no rest to either mind or body. Nevertheless, the great utility in general of such intellectual influences is not to be denied. The agreeable intellectual impressions, the recreation and amusement of the mind, the light and desirable mental occupation connected with the journey, the novel residence, and the new life, may, in many cases, act even more powerfully on the patient and the course of his disease than the climate itself, and may compensate for, and render harmless, some injurious influences of the latter.

It would, of course, be very desirable, in the examination of the influence of southern climates on diseases, to have at our command a large mass of empirical information. When we review the records of medical experience with respect to the result of a residence in the so-called southern climatic sanatoria, we certainly find such in reference to chronic bronchial catarrh and chronic pulmonary tuberculosis. But these records proceed from very different sources, and are in part very vague and inexact. Here, too, therefore, careful criticism is required; and we must first ascertain what records are in general available, and how far they justify us in coming to a conclusion as to the therapeutic importance of a southern climate.

The value of observations as to the curative effect of a southern climate in chronic bronchial catarrh and chronic pulmonary tuberculosis has hitherto not been highly estimated, because these two diseases are so frequently confounded, the differential diagnosis is so seldom accurately established, and, in fact, in many stages of these diseases, and especially in those in which a curative effect might most reasonably be expected, it is not to be made with certainty. With respect to the cases on record of cured tuberculosis of the lung, we very frequently hear a doubt expressed, whether the disease was really pulmonary tubercle. But if we take into account only the instances where the differential diagnosis was established beyond all doubt, and include only the cases of pulmonary tuberculosis where ulcerative destruction of the tissue of the lung was objectively demonstrated, or the signs of general phthisis were distinctly manifest, not only would the whole mass of evidence become very scanty, but this would comprehend only those cases, where, from the advanced stage of the disease, a cure was scarcely to be expected, and where any journey and change of place must be a matter of serious consideration. I do not look upon the possibility of confounding the two diseases as of so much consequence in its bearing

upon the subject under consideration, and I believe that even those cases where the differential diagnosis is not fully established, nevertheless supply an available material, because the treatment of both affections is in so many respects the same. We have, in the treatment of chronic tuberculosis, often no other object than the accompanying bronchial catarrh; and if we have been able to allay, relieve, or prevent this, we have done much. Therefore most remedies employed in chronic bronchial catarrh are likewise recommended in chronic pulmonary tuberculosis; and we might infer that when a patient labouring under chronic bronchial catarrh is well in a given climate, this climate will also be favourable to a patient affected with pulmonary tuberculosis. In my opinion, the improvement of the general state is much more important in reference to the availability of observations, than the placing of the diagnosis beyond a doubt; and a local diagnosis is certainly sufficient to enable us with probability to infer the presence of pulmonary tuberculosis, or of a mere simple catarrh.

It would further be desirable that we had at command a certain number of cases to supply us, to some extent, with a guarantee that the "*post hoc*" coincides with the "*propter hoc.*" I know very well that the greater number in themselves do not prove much, and a well-observed case is worth more than a hundred superficially recorded. But, nevertheless, a greater number of medical observations is here very much to be desired; for, when a patient leaves his home, to seek, in a strange land and in another climate, for restoration to health, he is liable to so many and various influences, other than climatic, which may contribute a favourable or unfavourable effect, that it is often difficult to decide what share the climate has had in the result. Frequent observations are necessary to enable us to say that patients, under the several circumstances and accessory influences in a certain climate, have reaped decided benefit, and that the climate has been the principal means of procuring the same.

If we now ask whether numerous and reliable records exist as to the curative effect of a southern climate, we shall obtain no very satisfactory answer. Although hundreds of invalids yearly travel to the south in order to regain their health, we possess only few exact and reliable medical observations on the subject; on the other hand, a number of opinions have been formed, which have, however, been based upon an unscientific application of experience. Many patients travel to the south without having consulted a medical man. Medical men, also, have often neglected to establish the diagnosis and the necessity for change of climate, and have rather yielded to the patient's inclination or his longing for recovery. If such patients then return cured or worse, or die at the place of their resort, where they expected health, premature conclusions are very frequently drawn from such results to the general efficacy of a climate. How often do not patients undertake a long journey to the south in a condition in which any journey is a risk. Of this I witnessed a striking example. In the winter of 1856-57, an English invalid

came to Egypt, who died of exhaustion in the railway train on his journey from Alexandria to Cairo, the place of his destination. A striking recovery, or an unfavourable result, if it acquire importance from the person to whom it occurs (it is sufficient to refer to the visit of the Empress of Russia to Nice, and of the French actress, Rachel, to Egypt), has often brought a climatic sanatorium into good or bad repute, while, perhaps, hundreds of other cases could have shown the unfounded nature of such credit or discredit, though, because they did not accidentally excite some general interest, they remained unknown or unattended to. Such unwarranted opinions have found acceptance both with the public and with physicians, but only very few certain medical observations have been made known.

With respect to the utility of the several empirical observations, it must also be considered how such a residence should be carried out. On this point mistakes are often made with and without the fault of the patients. Many invalids travel to the south, but do not procure, or fail to attend to, the necessary instructions for their new life, live unsuitably with regard to food, clothing, dwelling; or adopt an unhealthy occupation. Many do not wish to have travelled to Italy or Egypt without having seen the far-famed historical memorials and art treasures of these countries, and thus are led to use too great bodily exertion, and to travel from place to place, while their mind is kept in a state of constant agitation and excitement. In such patients the effect of a southern residence can scarcely be favourable, though it can prove nothing against the beneficial influence of the climate.

Of the injudicious use of a southern climate, the following case, furnished by my own experience, may serve as an example:—In Cairo I met with a North American, who came thither in January, 1858, labouring under pulmonary disease, and in a miserable condition. The first part of the winter he had spent in Italy, had travelled much about there, and in that short period had visited the principal cities of that country. In Cairo I lived with him in a villa close to the Nile, a league from the city properly so called. The journey to the city he very often made on an ass, which appeared to me to be a very great exertion for him; but he made still greater and more fatiguing tours. Although I never subjected this patient to a strict medical examination, and only occasionally had social intercourse with him, I was convinced, by the most striking signs, that he laboured under pulmonary phthisis, as, together with excessive general emaciation and weakness, he had a violent cough, copious viscid expectoration, often mixed with blood, and at the same time complained of feverish symptoms, sleeplessness, and dyspnœa. Besides the bodily exertions which he exacted from himself, I must mention the unsuitable diet he adopted; he drank strong wine, often brandy-and-water, ate, fruit, &c. In the following April I met him again on the voyage from Alexandria to Malta; he complained then, with good reason, that his residence in the south had

been of no use to him; he was in such a wretched state, that he had to be carried up and down the stairs in the steamer. In this instance the unfavourable course of the disease is certainly to be ascribed only to the injudicious use of the climate, and not to the climate itself.

Another striking example I met with in a young Hungarian. This patient had been sent to Cairo on account of pulmonary disease, and had there spent a year and a half. During his residence in Cairo he had made frequent and long excursions on asses, horses, and camels, and had spent much time in shooting and hunting, his game being chiefly waterfowl and wild boars. He remained two winters, from 1856 to 1858, and, imprudently, the intervening summer also in Cairo; he then made, in April, a journey to Jerusalem, from Alexandria to Jaffa in the steamer, from Jaffa to Jerusalem on horseback, a journey which cannot be performed without bodily fatigue. In the month of May following, I met him atNaples, where he consulted me professionally. He was, of course, worse. On accurate examination, I found the objective signs of infiltration and cavernous formation in the lungs, and the best marked signs of general phthisis. Exhausting attacks of diarrhœa had, moreover, supervened. Dreading the injurious effects of a southern summer in such a state of debility, particularly in chronic diarrhœa, I recommended him immediately to leave Naples, and either to return home, or to take a summer residence in an Alpine region. But, in spite of my frequent and urgent admonitions, the patient remained the greater part of the summer in Naples, made excursions, visited galleries and similar sights, and ate and drank whatever he fancied. Meanwhile he became steadily weaker. Finally, towards the end of July, he determined to leave, and called upon me in such a state, that I felt obliged to endeavour to dissuade him from taking any journey. I recommended him to stay quietly in Sorrento, as being cooler, Capri, or some similar neighbouring locality. But he was now as little to be kept back as he was before to be moved to take his departure. A true mortal agony seemed to urge him on. He set out in the steamer for Genoa. In embarking he had to be carried on deck. When the steamer returned to Naples, I inquired of the officials what had become of the unfortunate patient. I was informed that when the vessel arrived at Genoa, he was found dead in his berth. Neither can this case, on account of the absurd abuse of a southern residence, prove anything against the efficacy of the climate.

Though only few accurate and really useful reports of cases are to be found in medical literature, still we meet with some observations where the diagnosis, stage, and course of the disease, are scientifically given. And these observations are strongly in favour of the beneficial influence of a southern climate on chronic bronchial catarrh and chronic pulmonary tuberculosis. We find even cases described, where the subsequent post-mortem examination might

prove its favourable curative influence. Besides, general opinions and statements from competent sources, as to the efficacy of the southern climatic sanatoria exist, which likewise depend on empirical experience, without the particular and detailed observations being directly given. In such cases it is, of course, necessary to inquire into the competence of the persons giving the opinions, and only to admit such of the latter as are really reliable.

Another mode has been adopted of scientifically determining the importance of a southern residence for patients affected with pulmonary tuberculosis. Thus it has been attempted to make the geographical distribution of tubercle of the lung, the rarer or more frequent occurrence of the latter in the climatic sanatoria, a measure of the influence of the climate in question upon this disease. It is certain that the statistics of disease and mortality in a country ought to furnish important data for the medical estimation of a climate. But the statements which exist as to the relative mortality of those places must be used with circumspection. The medical statistics published for these localities have been freely used and taken as the groundwork of investigation. But they are neither reliable, nor do they admit of extensive conclusions; for, if we look at the hitherto political, social, but especially the medical circumstances of those places under consideration, we can expect no accurate and certain statistical figures. Moreover, these statistics do not comprehend the entire population, but usually only a small fraction of it—the inmates of hospitals for the poor, and the part of the inhabitants who are in general exposed to the more unfavourable conditions of life. But if we should admit that reliable and sufficient medical statistics existed, these would warrant only a conclusion as to the influence of the climatic conditions on the residents, who are exposed to them during the entire year. Now, when we send our patients to a southern climatic sanatorium, it is only for a sojourn during the winter season. The injurious effects of the southern summer do not reach them. Certainly, in modern times, when a southern residence has been extended over several winters, patients have been advised to remain in the south also during the summer, on account of the difficulty of getting home; but in that case the higher mountains of the countries in question, where the temperature is much cooler, have alone been considered adapted to the purpose.

Just as little do the statistical investigations instituted as to the occurrence and circumstances of chronic pulmonary tuberculosis in the French Navy[a] prove anything against the climatic sanatoria. These investigations have shown that the proportion of those who die of that disease in the navy is to the other cases of death as 1 to 7.59, almost twice as unfavourable a result as in the land army, where this is as 1 to 13. Hence the practical rule has been

[a] J. Rochard, *op. cit.*

deduced, to warn all young people who have a tendency to pulmonary tuberculosis, or who already present the incipient symptoms of the disease, against adopting the profession of a sailor. This conclusion may be justified. Considering that the stations of the French navy are, with very few exceptions, situated in a southern climate, it has been attempted to draw the further inference that a southern climate in general is injurious to those labouring under, or predisposed to, tuberculosis. But this inference is neither generally correct, nor does it prove anything against the so-called southern climatic sanatoria. For the sailor is exposed to many influences prejudicial to health, among which the many changes of weather, the limited space and crowded residence on shipboard, are to be enumerated. These and similar disadvantages press specially upon the calling of a seaman, but do not reach him who visits the south on account of his health. Moreover, the stations of the French navy lie in great part in the torrid zone, in the vicinity of the equator, in the East Indies, Africa, Senegal, the West Indies, South America, and in the Pacific Ocean; while the southern climatic sanatoria are in the temperate zone, on the shores of the Mediterranean Sea, in the neighbourhood of the latter, or in similar geographical situations. Finally, those investigations relate to the life of a sailor in a southern climate during the whole year, while a patient remains in a southern climatic sanatorium only during the winter.

While we must by no means infer that because pulmonary tuberculosis is of frequent occurrence among the inhabitants of a southern climatic sanatorium, the influence of the climate of the latter will necessarily be unfavourable to those patients who come thither from the north to spend a milder winter than they could enjoy at home; we may conclude, on the other hand, that the influence of the climate of those places where pulmonary tuberculosis is of rarer occurrence, will be beneficial to such invalids. Thus when, notwithstanding the unfavourable effects of the excessive heat of the southern summer, this disease is, in a certain locality, little met with, we must suppose that the conditions for the development of tubercle of the lung exist in absolutely less proportion, and that our patients must there meet with a climate so much the more favourable to them, as they are to spend in it only the mild winter. And there are, in fact, southern climatic sanatoria, where pulmonary tuberculosis is of rare occurrence. The places especially pointed out as such are Venice, Madeira, Algiers and Cairo[a]. Moreover, for most of these climatic sanatoria experience shows that even if, with respect to some localities, it cannot be proved that pulmonary tuberculosis is of rare occurrence, the disease has a very protracted course among the inhabitants. But the most im-

[a] See, for Venice, Sigmund's "Südliche Klimat. Kurorte;" for Madeira, Mittermeier, *op. cit.*; for Algiers, Castalat, Bertherand, "Gaz. Hebdom." 1858, p. 230, Haspel, "Canstatt's Jahresbericht," 1853, ii., 156; for Cairo, the writings of Pruner, Griesinger, Reyer.

portant empirical evidence of the suitability of a residence in the southern climatic sanatoria for patients affected with tubercle of the lung is that already mentioned—namely, that people who migrate from a southern to a northern climate are very likely to be attacked with pulmonary tuberculosis; while those who change from the north to a southern country rarely become tuberculous, and northerns so affected very frequently recover in the south. This is most strikingly seen in Egypt, where there is always a great confluence of northerns and southerns. The Arabs and Copts who have migrated from the southern parts of Egypt to Cairo, the Berbers of Nubia, and, above all, the Negroes of the south are, in Cairo, very frequently attacked with pulmonary tuberculosis; on the other hand, the Europeans who have migrated to Egypt are very rarely so affected; and those who have come from Europe to Egypt with a predisposition to the disease, or with well-marked symptoms of the same, are frequently improved or cured; indeed, it is said that Negroes, who have taken ill in Cairo, recover when they return to the south.

After these critical observations, I may be permitted to bring forward the positive results of the investigations as to the therapeutic importance of the southern climatic sanatoria, which appear to be best ascertained and best founded, and which, at the same time, present the greatest amount of practical interest.

Residence in the so-called southern climatic sanatoria may certainly be considered, in general, as a remedial measure in chronic bronchial catarrh and chronic pulmonary tuberculosis, for patients from the northern and central parts of Europe. On the other hand, it cannot be maintained that a southern climate is invariably, and under all circumstances, favourable to such patients. In the first place, tropical heat is often very destructive in its influence on the development and course of these diseases. Summer, too, in these sanatoria is, for the most part, injurious to the above-described patients. Only those localities can be considered as southern climatic sanatoria favourable to patients from our latitude, which are situated in the south of Europe, the north of Africa, or in similar geographical positions. These should, moreover, be used only as winter residences. According to our present climatological knowledge and medical experience, the following places are to be considered as the most favourable southern climatic sanatoria:—Malaga, Hyères, Nice, Venice, Pisa, Rome, Palermo, Cairo, Algiers, Madeira. After these, because less known and less recognised as sanatoria, are to be named: Cannes, Villafranca, Mentone, San Remo, Naples, Messina, Catania.

The efficacy of a residence in the southern climatic sanatoria is to be ascribed specially to the greater warmth of the southern winter. By removing a patient labouring under pulmonary disease to one of those southern localities a double advantage is obtained—first, the negative, that the patient escapes the injurious influences of the

northern winter, whereby his lungs and constitution are afforded an opportunity of recovering; secondly, the positive and direct, arising from the frequent enjoyment of a mild and pure atmosphere, and the power of taking bodily exercise, advantageous alike to the respiration and to the nutrition of the body.

Besides the greater warmth, other climatic elements also contribute to the cure, especially a certain regularity and limitation of the alternations of temperature, the purity and moderate movement of the air, as well as the rarity of rain. The amount of moisture in the atmosphere may likewise play an important part; but it cannot be truly asserted that a definite degree of atmospheric moisture is particularly favourable in all cases of these pulmonary diseases; it is rather to be assumed that, in some instances, a moister, in others a dry, climate is advantageous. What cases are better suited to one climate or the other, we have as yet no certain indications to show; but from what has been practically and theoretically established with respect to the several sanatoria, we may consider a moist, moderately warm climate to be, in general, a soothing and gentle expectorant in chronic bronchial catarrh and chronic pulmonary tuberculosis; and a dry, moderately warm climate to be secretion-limiting, tonic, gently-exciting, and specially promotive of the general nutrition. The former would, therefore, be more suitable to cases of these diseases accompanied by a sensitive state of the bronchial mucous membrane, viscid expectoration, frequent and dry cough, and an easily excitable condition of the vascular and nervous systems: the latter to the torpid form of the disease, with abundant expectoration, and to patients in whom the principal indication is to strengthen the system at large.

The several climatic agents may be grouped in different ways, may cooperate or counterbalance one another, and so, in various modes, contribute to the cure.

Besides the climatic, other influences are to be considered, deducible from change of locality, social relations, occupation, and the whole mental and bodily mode of life. These influences, it is true, possess only an individual importance; but they may, under some circumstances, play a very important part.

In conclusion, I shall collate the southern climatic sanatoria whose climates are best understood, according to their mean winter temperature and their winter conditions of moisture and rain, placing them so as to form ascending series.

N.B.—The following numbers are the results of mean calculations based upon observations continued, in the majority of instances, during many years, and are borrowed from the monographs upon these sanatoria already quoted—Mittermeier's upon Madeira, Reil's on Egypt, Sigmund's on the sanatoria of Northern Italy, and especially Vivenot's on Palermo, in which latter work the most numerous and most complete tables are to be found. The position in the series, when it is not justified by numbers, is based upon the

explicit opinions of these and other competent authors (Bertherand on Algiers, Francis[a] on Malaga).

### I. SERIES,

Exhibiting the mean temperature of the winter—that is, of the months of November, December, January, February, and March:—

| | |
|---|---|
| Venice, (40°.59F.). | Naples, (51°.53.F.) |
| Hyères, —— | Palermo, (54°.29.F.) |
| Florence, —— | Catania, (54°.83F.). |
| Pisa, (45°.79F.). | Cairo, (58°.64F.). |
| Nice, (47°.12F.). | Malaga, (59°.F.) |
| Rome, (48°.62F.). | Algiers, (61°.06F.). |
| Cannes, —— | Madeira, (61°.47F.). |
| San Remo,—— | (Funchal) |
| Mentone, (50.°0F.). | |

In the case of Malaga and Algiers, the temperature is calculated for the winter, including October.

### II. SERIES,

Representing the mean atmospheric moisture in winter:—

| | | |
|---|---|---|
| 1. Cairo. | 6. Mentone. | |
| 2. Algiers. | 7. San Remo. | |
| 3. Hyères. | 8. Naples. | |
| 4. Nice. | 9. Rome. | |
| 5. Cannes. | 10. Pisa. | |
| 11. Madeira, 75.4°/₀. (Funchal). | | Mean for Nov., Dec., Jan., Feb., and March. |
| 12. Palermo. 76.2°/₀. | | |
| 13. Venice, 80.4°/₀. | | |

### III. SERIES,

Showing the number of rainy days in winter—that is in November, December, January, February, and March:—

| | | | |
|---|---|---|---|
| Cairo, | 13.7. See Series IV. | Madeira, (Funchal). | 55.7. |
| Malaga, | —— | Florence, | 57.4. |
| Nice, | 25.7. | Rome, | 58.7. |
| Venice, | 28.1. | Pisa, | —— |
| Algiers, | —— | Palermo, | 65.7. |
| Catania, | 45.3. | | |

[a] [A review of Dr. Francis's work on Change of Climate may be found in the sixteenth volume of the present series of this Journal, August, 1853, page 147.—TRANSLATOR.

IV. SERIES,

Arranged according to the amount of rain in winter—that is, in November, December, January, February, and March:—

| | | | |
|---|---|---|---|
| Cairo, | scarcely measurable. | Naples, | 413.90 millimetres, |
| Venice, | 229.99 millimetres. | Madeira, | 548.44 ,, |
| Palermo, | 364.06 ,, | | (Funchal). |
| Rome, | 407.94 ,, | Pisa, | 548,90 ,, |

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*On the Treatment, without excision*[a], *of Wens and of some other Cysts.*
By M. A. COURTY, Professor in the University of Montpellier.

To speak of wens is to touch upon one of the most humble and common subjects in surgery; for there is no application of our art more frequent, and in some measure more routine, than the treatment of those tumours, often so numerous, and of dimensions so varied, which are developed on the head, in the substance of the scalp, or below it, in the neck, on the face, and some other parts of the body, consisting of a cyst and its contents, whatever may be the variable thickness and density of the envelope, the solidity or fluidity of the encysted matter.

Removal is a prompt and easy method of dealing with them; but in addition to the fact that it is not always exempt from accident, it is not readily consented to by patients. Not to mention the numerous cases in which the multiplicity of the wens makes the patients dread the renewal of the pain they have once unwillingly consented to undergo, these tumours being naturally painless, often little troublesome, not interfering with any function, not threatening life, nor causing imminent danger nor real infirmity, it is natural that they should be borne, until their great development, inducing the inconvenience of a disagreeable deformity, or the danger of approaching degeneration, overcomes the timidity of the patients, and hastens their determination to submit to operation. It still remains for us to combat the prejudice entertained, that in touching a wen we may run great danger, not only of such local accidents, as erysipelas, inflammation, or epithelial development, which are real; but still further by the possibility of supposed metastases, or of some disturbance which their removal may excite throughout the system.

These considerations have induced me to say a few words on the development of these tumours, to show that it might always be prevented, if advice were sought in time, and to describe the mode of treatment which I have found most successful.

[a] Men who have attained to eminence in the science of medicine have often been reproached with neglecting the discussion of common, though important subjects The proverb, "*De minimis non curat prætor*," shows that this reproof is of ancient date. Professor Courty appears anxious to make an exception to the rule; in this he sets a good example: we, therefore, hasten to reproduce his essay on the Treatment of Wens by Cauterization.—ED. *B. G. de T.*

Under the name of wens are included subcutaneous, indolent, circumscribed, encysted movable tumours, susceptible for the most part of acquiring a considerable volume, and containing a fatty or oily matter. Cyst and fatty matter are the two terms, the union of which may be regarded as characteristic of every wen (*loupe*). As fatty cystic tumours, wens must be distinguished anatomically, physiologically, and surgically—on the one hand, from other fatty tumours; on the opposite, from other cystic tumours.

The other fatty tumours are lipomas, true local hypertrophy of the adipose tissue, capable of producing equally all the elements of that tissue, or only some of them, or some more prominently than others, sometimes complicated with the addition of new fibro-plastic productions, or by the coexistence of crystallization of fatty matters (cholesteatoma). They ought to be called fatty, rather than fat, tumours. Far from being confined in a cyst, they are usually movable, ill-defined, insinuating themselves into the interstices of the tissues; and unless they are pediculated, having generally no boundaries but the aponeurotic envelopes which separate the different planes, or which surround an organ. They present a tuberculated aspect, prolongations, supposed roots, a characteristic softness, a deceptive fluctuation. Thus they require dissection, the only method which enables us to remove them totally, and to avoid the dangers inherent in other modes, of leaving behind portions, or of promoting the formation of purulent sinuses; and sometimes the source of the adipose tissue is at a distance more or less great from the seat of cauterization, of the seton or the ligature. We are sometimes astonished at the distance to which we are led by dissection to follow the excentric lobes of lipomas between different muscular planes; I have removed some on the chest which insinuated themselves under the great pectoral. A few months ago I removed an enormous lipoma, the prolongations of which I had to follow to the deepest plane of the abdominal muscles. I have lately seen another, insinuating itself under the trapezius and rhomboid, as far as the spinal muscles.

The other cystic tumours may depend, like wens, on the dilatation of a cavity accidentally or naturally closed, by a normal or abnormal liquid secreted in this cavity (mucous cyst, hygroma, ovarian cyst, ranula, &c.); but they may depend also on the formation of an envelope around a sanguineous extravasation, a foreign body, an entozoon, or a cancerous formation. In a word, there are between the containing and the contained variable relations of preformation and postformation, which affect not less than the nature itself of the tumour, the determination of the indications to be fulfilled to accomplish a cure.

As to wens, whatever be their dimensions, there is, in the contained and the containing substance, a constancy of composition and of origin, which makes a perfectly natural order of tumours, the order of sebaceous tumours or cysts.

The containing organ, that is to say, the cyst, is formed by the thick walls of the simple sebaceous clustered glands, which are the starting point of the development of the wen. It therefore exists previously to this development, and differs from itself, in its different periods, only in the transformation it has undergone. The orifice of the excretory canal becomes obliterated, or remains insufficient for the evacuation of the superabundant products of a vitiated secretion; it may even successively reopen and be obliterated several times, permitting the cyst to be alternately collapsed and distended, to empty and refill itself anew. The surgeon, when he discovers this orifice, usually recognisable by the existence of a black point, may make use of it for the evacuation of the contents, and for the ulterior treatment of the cyst by the method I am about to explain.

The contents, more or less liquid, sometimes solid, suety, or chalky, are formed of sebaceous matter, that is to say, of free fatty granulations, globules of various sizes, of little drops, or of an oily homogeneous liquid, of cells of pavement epithelium, often without nuclei, with or without granulations, often altered, plaited, shrivelled, some of them vesicular, ovoid, distended by their contents and detached from the inner surface of the gland; lastly, of crystals of cholesterine and of granulations, or of a sort of mortar of carbonate of lime or magnesia. In one degree, the tumour, known under the name of grub (*tanne*), is still small, and is filled simply with thickened sebaceous matter. In a second degree, it, more or less voluminous, includes sometimes a yellowish matter, having the consistence of honey (*mélicéris*), sometimes a whitish substance, occasionally resembling thick pus or pap (*atheroma*), which, from its flat form, or from some other circumstance, has received the names of *talpa*, *testudo*, &c. In a third degree, the tumour, very large, is formed by the accumulation of a fatty substance, having the consistence and colour of suet (*steatoma*); it is often accompanied with hypertrophy of the gland, with a tendency to the formation of an adenoid tumour, with a disposition to epithelioma, justifying the opinion of those surgeons who have pointed out the possibility of the cancerous degeneration of steatomatous tumours.

The precision of our knowledge of the nature of wens scarcely admits of our discussing the questions of the danger which may accompany their cure, of the possibility of a metastasis after their suppression, &c. It is to a certain point the same with respect to the treatment to which it has been proposed to submit patients, with a view to combat the lupial or wenny diathesis. Is there only diathesis? It is easy to answer this question in the affirmative, if we take diathesis in the sense of predisposition; for it is certain that in some individuals there is very great tendency to obliteration of the excretory canals of the sebaceous glands, to superabundance, alteration, thickening of their secretion, and consequently to the development of wens; but can we regard as a permanent morbid affection, what is, in fact, only a more or less frequent effect of the retention of secreted products?

On the other hand, the accuracy of our knowledge enables us to adopt the best surgical treatment of these tumours. Setting aside removal, which must sometimes be practised, it may be said that the destruction of the cyst is the key to every method, the application of which is capable of producing a permanent result. It remains to determine the best mode of destroying the cyst, and the cases to which the method is applicable.

Let me say a few words, first, on the removal of the tumour by means of cutting instruments.

This operation is usually indicated in cases of very voluminous wens, although, to say the truth, it may be dispensed with even in such cases. It is, on the other hand, more formally indicated for little cystic tumours of the eyelid,—being, moreover, in this particular instance, perfectly harmless. Lastly, it is applicable to wens of medium size, and may be performed with such rapidity as to give the patient scarcely any pain. To make an incision in the skin, and rapidly enucleate the cyst with a spatula, as has been so dexterously done by Dupuytren; or, after having divided the wen through the middle to its full extent, to remove, one after the other, the two halves of the cyst, seized with a strong forceps, as M. A. Guérin does, is to combine the certainty of success with the greatest operative rapidity.

I have often operated in this mode, and have always had to congratulate myself on having done so. Lately I removed by this process, in a young lady, and some days after in a man, two atheromas as large as nuts, situated in the lateral region of the neck, without giving the patients time to utter a complaint. The wound subsequently united, also, with great rapidity. It is, however, well known that the consequences of an operation, apparently so slight, may be aggravated by the development of erysipelatous inflammation, and even of phlegmonous erysipelas. We know that these accidents may be serious, especially when the dimensions of the cyst, its adhesion to the integuments, the thinning of the skin, oblige us to practise a more or less laborious dissection. We know, in fact, as I have already stated, that patients often refuse to submit to this mode of treatment.

These several reasons have led me to perfect the plan of proceeding by destruction of the cyst.

I shall begin by observing that the destruction of the cyst does not necessarily involve that of the skin. It is well to make this remark, because the majority of surgeons who have applied agents of destruction, especially of cauterization, in the treatment of wens, have applied them on the skin; differing not much in that from quacks, who, by means of so-called dissolving plasters, or of pretended chemical pencils, determine on the skin, throughout a variable extent, the painful formation of eschars more or less extensive, which may become, as well as incision, the source of inflammatory symptoms. Whether, with M. Legrand, we employ linear cauterization with caustic potash, or, with Bonnet, superficial cauteriza-

tion with Vienna paste, or pâte de Canquoin, or, with M. Maisonneuve, *cautérisation en flèche*, &c., the skin is always attacked. I have had occasion to see this cauterization followed by erysipelas; and even when the bottom of the cyst was thus exposed, without being destroyed like the surface, I have witnessed an epithelial vegetating production developed at the bottom, and soon assuming the aspect of an epithelioma, or of a cancroid, little encouraging to the operator, and necessitating a fresh and extensive application of the caustic.

I, therefore, lay it down as a principle that the skin ought to be, so far as possible, preserved, and that the destructive agent ought to be carried into the very cavity of the cyst.

In what mode are we to penetrate into the cyst?

Almost always in grubs and little wens, often even in voluminous tumours, we find a black spot indicating the seat of the neck oft he orifice, which is supposed to be necessarily obliterated, and which is often only plugged. Nothing is easier than to remove the obstruction by pressure, or by the action of some very simple chemical or mechanical agent, a little soap and water, alkaline solution, the point of a probe, of a pin, or needle. We empty the cyst by pressing it; sometimes the thickened matter comes out gradually, in the form of a worm; sometimes, more fluid, it escapes in a jet, and is quickly and entirely expelled by the pressure of the fingers.

If this orifice is deficient, the point of a needle, of a little trocar, readily and painlessly substitutes for it an artificial opening.

Finally, if the cyst is very voluminous, if it is multiple, as I have seen in several instances, a double puncture, kept up by the presence of a seton during a few days, suffices to afford access to a single or multilocular cavity, and to give time and opportunity to express by degrees the entire contents, without exciting pain, or giving rise to any inflammatory symptom. To make this double puncture, and introduce the seton, I prefer the trocar to the needle, the more rapid and less painful introduction of which forms openings larger, and less sensible to the contact of the thread. I then perforate the tumour through and through with a trocar, leaving the canula some moments *in situ*. Into this I pass the seton, which, therefore, does not touch the openings; then I withdraw the canula, and fix the seton by attaching its two extremities to each other. When the tumour is very voluminous (and I have been able thus to operate on some which were as large as a turkey-egg, or as an orange), I make use of M. Chassaignac's drainage trocar, the point of which, furnished with a notch to which the thread may be attached, allows the tumour to be perforated, the seton to be passed into the canula, and the latter to be withdrawn in less time than it takes to describe this little manœuvre.

The cyst having been emptied, how must we proceed to effect the obliteration of its cavity?

Strictly speaking, we may have recourse to one of two modes:

either to modify its inner surface, to irritate it, inflame it, and determine by approximation and contact the adhesion of the opposite walls; or to produce its more or less rapid destruction, and its expulsion by the orifice, the cutaneous envelope quickly retracting, and gradually filling up, either by retraction or by deep adhesions, the void caused below it by the expulsion of the cyst. This latter mode is far preferable to the former.

At first, that is to say, about fifteen years ago, I was content to aim at simple modification of the walls of the cyst. To this end, after having expelled all the contents, I injected into the cavity various liquids. I had adopted, in preference to all others, a strong solution of caustic potash (one part, and sometimes more, to fifty parts of water), thrown into the cavity by means of a small glass syringe. This solution had the advantage of first dissolving all the fatty or sebaceous matter which remained adherent to the walls, and then of irritating the latter, and disposing them to adhesion. Methodical pressure, continued or interrupted, favoured by the natural tendency to retraction of the skin and of the cyst, which were no longer distended by their contents, brought about by degrees the adhesion of the several portions of the sac, which eventually came into mutual contact, and the radical cure was finally attained after the lapse of a period varying from about fifteen to fifty days. On no occasion did pain or any other accident interfere with the treatment.

Since that time, thanks to the constant progress of cauterization applied to surgical operations, I gradually substituted for the caustic potash—of which I no longer made use, except to completely wash out the sac—agents more energetic, and more easily managed. Canquoin's paste, and particularly his various sparadraps, such as the school of Lyons has introduced into ordinary use, appeared to me perfectly to fulfil the object I proposed, and I have not ceased to employ them in the treatment of wens.

The following is the mode of proceeding:—A piece of sparadrap of chloride of zinc is cut of a length proportioned to the capacity of the cyst, narrow enough to pass through the opening; its penetration may be facilitated by rolling it into a cylinder, so that the caustic substance occupies the external surface of the long and slender cylinder. It is always easy to push it into the interior, the natural orifice of the wen admitting of its passage with the slight aid of a director, a probe, or the head of a pin; the artificial opening, if it exists, being so much the more convenient, as it must have, as we make it, more considerable dimensions. For grubs, a piece of Canquoin as small as a grain of millet, or a lentil, is sufficient; for the more voluminous wens, a cylinder from one to three centimetres in length will be required. If the cutaneous opening appears insufficient, the end of the caustic is retained in it, instead of being forced entirely into the cavity of the cyst.

Pressure exercised on the wen at least twice a day gives exit,

from this time, to a purulent matter, occasionally accompanied with small shreds of the cyst, and remains of the caustic sparadrap. But here two cases present themselves: either the cyst is small, very movable under the skin which covers it; or it is very large, and adherent to the tissue in a great number of points.

In the first case, that is to say, when it does not exceed the size of a large nut, which is not so very small, one cauterization is often sufficient to effect a cure. Whether this takes place after one or several introductions of the caustic, it is usually accompanied with the expulsion of the modified cyst. By the use of pressure, the latter makes its appearance at the opening, whence it is easy to complete its extraction with the forceps, when its expulsion does not occur spontaneously. We then obtain the cystic sac intact, the dead fibrous tissues presenting a whitish or gelatinous appearance. It is unnecessary to say that the complete obliteration of the cavity, the retraction of the skin, and its adhesion to the deep-seated parts, soon follow upon the expulsion of the sac.

If patients were well-informed as to the facility of freeing them without pain or danger from these grubs and little atheromas, which threaten to become large wens, we might with certainty prevent the development of every tumour of this kind. To introduce through the natural opening of the grub, or through an artificial opening made with a needle, after having evacuated its contents, a small portion of caustic of the size of a lentil, and thus to destroy the cyst, the expulsion of which takes place almost spontaneously, would be a matter of a few days. We should thus always prevent a small, indolent tumour from ever becoming an inconvenient or painful deformity; and we should avoid exposing the patient to the sufferings and dangers of an operation, which at last becomes indispensable.

In the second case, that is to say, when the cyst is voluminous, of long standing, multilocular, &c., we cannot hope for a termination so rapid or so complete. We must then reckon on the successive expulsion of shreds of the cyst, which gradually mortify, or on the adhesion of the points of the sac which shoot out sufficiently, in consequence of the irritating action of the caustic, to unite one to another. The successive introduction, at some days' interval, of several pieces of Canquoin's sparadrap, methodical pressure exercised on the wen either continuously or interruptedly, care to keep it always emptied of the products secreted in it, and to favour the retraction of the skin, and the contact of the opposite surfaces, finally bring about by degrees a result as advantageous as that obtained in the first case at a less expenditure of time and money.

The duration of the treatment may then vary from thirty to fifty days, but the result is always cure; and the patients attain this end without having incurred the least danger, or experienced the slightest pain. Contrary to what is true of the skin, the inner surface of the cyst has always appeared to me to be insensible to the action of caustics, all the benefit of which is consequently obtained,

without our having to fear the reactions which follow the development of the pain inevitably connected with their application to the cuticle.

I have never observed, in the very great number of wens I have in this mode operated on, the production of a single one of the accidents which may follow any surgical operation.

The large number of cases which I should have been able to quote in support of the efficacy and harmlessness of my method of treating wens, the uniformity and monotony of these details, have decided me to bring forward none, each particular observation having been the exact reproduction of the general history I have just traced.

It is scarcely necessary to add, that certain mucous cysts, slightly-developed hygromas, &c., may be treated as I have done, with as much success, by the same method.—*Bulletin Général de Thérapeutique*, 15th June, 1860, page 490.

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*On the Local Action of Savin.* By DR. EISENMANN, of Wurzburg.

THE Hungarian Journal of Physiology and Medicine, 1859, No. 5, and from it the Medico-Chirurgical Monthly Papers for May, 1859, contain a communication by Dr. Moller, entitled, "Savin as a Popular Remedy," according to which it would appear that in Hungary a decoction of savin is in common use for the removal of polypi. The author is acquainted with the case of a peasant woman, who by means of this agent got rid of a nasal polypus, and he himself successfully employed it to destroy and remove a soft nasal polypus which he had twice in vain extracted. On account of the softness of the polypus, he had not been able to extract the root, and it had consequently always again grown rapidly. The following is the mode of using it:—A drachm of savin tops was boiled for five minutes in about three or four ounces of water, and the polypus was painted frequently during the day with the filtered decoction; the fluid was also injected into the nose. This communication reminds me of some results I have myself observed from savin. In Hecker's old Materia Medica, the following preparation is found under the name of Hecker's salve. An onion is roasted in the ashes; the juice is expressed, and with this juice and a corresponding quantity of powdered savin, a sort of liniment is prepared. This liniment is recommended by Hecker in the treatment of condylomata, and in fact it removes venereal warts in a surprisingly short time; during twenty years' experience I repeatedly observed very fully developed condylomata disappear completely after three days' use of this ointment. These remarks may serve to introduce the following.

During my stay at the fortress of Oberhaus, at Passau, I became acquainted with Bonkratz, the drummer of the battalion, who attracted my attention in consequence of a tumour he had on his neck, beneath the right ear. This tumour was circular and flat, was equal

in circumference to a crown-piece, rather over than under it, and was about a quarter of an inch in thickness. It had a tolerably wide base, was hard as fibro-cartilage, and presented a bluish red appearance. This tumour had been frequently removed with the knife, and its root extirpated by the actual cautery, but it had always grown quickly again. In other respects Bonkratz was in good health, had no traces of syphilis, and his skin was free from eruptions and ulcers. The causes of the tumour were quite unknown. I spoke to Dr. Delzer, at that time surgeon to the regiment, about the case, mentioned my observations on the efficacy of savin in the treatment of condylomata, and suggested to him to make a trial of Hecker's salve with this swelling, which resisted all other remedies. Dr. Delzer, whom I always found extremely friendly, agreed to my proposal, and promised to carry it out. However, in September, 1843, I left the fortress, and lost sight and recollection of Bonkratz. In the autumn of 1847 I met the same Bonkratz at Nürnburg, where he had been employed at the custom-house, quite unexpectedly in the street. The tumour in his neck was completely gone, and, as he said, in consequence of the use of the remedy I had recommended.

Savin, therefore, exercises a remarkably deletery influence on condylomata, polypi, and tumours, which may perhaps be regarded as polypi of the external skin, and who knows how much further this influence may extend?—*Virchow's Archiv*, Band xviii., 1 and 2 Heft, page 171.

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*On the Influence of Warm Climates on Phthisis.* By PROFESSOR FORGET, of Strasburg.

THE question of climates as causes of certain diseases, immediately suggests the idea of their curative power with respect to other affections. It is, in fact, generally admitted that the diseases produced by cold, being rarer in hot countries, ought also to be cured by residence in hot countries; the converse is not equally true, for atmospheric cold produces many more diseases than it can cure. However, it is admitted that dysentery, for example, produced in hot countries, improves under the influence of a climate not cold, but temperate.

According to these principles, it was, until latterly, universally admitted, that the diseases of the chest produced by cold, phthisis in particular, were favourably modified by residence in hot countries; but the love of innovation, and the rage for the paradoxical, which have seized upon the medical heads of our epoch, have succeeded in changing all this; and the Academy of Sciences has just crowned a work, the conclusion of which is, that a sojourn in warm climates favours the production and development of phthisis. The author goes further, he explains why heat is prejudicial to tuberculous patients: it is, he says, because it engenders the debility which itself presides over the generation of phthisis. Behold, then,

the question judged by a formidable array of statistical documents, which seems to leave no room for discussion.

Well! I declare this to be a gross illusion—I do not say an error; for, even admitting the fact, which is not fully proved, I undertake to explain the most rudimentary according to the simple laws of pathology. This is not a quibble; for in admitting, *in globo*, that hot countries are fatal to phthisical patients, you take from the latter all desire to go seek relief under a milder sky than ours; while, by learnedly interpreting the pernicious effects of hot countries, you teach patients to preserve themselves from these effects by profiting by a salutary climateric influence.

And, in the first place, to return to our starting point, it is, *à priori*, extremely probable that phthisis produced by cold ought to be relieved by heat. So says common sense.

Let us now consult experience. In like manner as patients set little value on simple and indigenous remedies, and place their confidence in those that are expensive, physicians affect a decided disdain for natural scientific processes, within the reach of all understandings, and take pride in searching in darkness for truths which strain their eyes. This eccentricity of mind has other springs than simple pedantry; and, among other causes, it results from the horror of rationalism implanted in the minds of the rising generation during the last thirty years. Instead of investigating with so much labour documents borrowed from the English records in both the Indies, instead of sending at a great expense experts to Africa or elsewhere, it would have sufficed, I think, and would have been right, first of all to examine what passes around us.

It has been justly said that seasons are transitory climates; and every one knows that our summers often present a temperature equal to that of inter-tropical climates. In fact, even the equatorial heat is difficult to bear by its continuity, rather than by its excess. Well, how does phthisis go on during summer? And, first, the fact is that unfortunate consumptive people dread cold, dread winter, and justly so, and they expect the return of warmth as the convict does his pardon. As soon as the vernal sun shines forth, they believe they may take their fling, and expose themselves to those variations of temperature which render that season so fatal. It is only in summer that they breathe at leisure, and in security; but on one condition, however, that they avoid the influence of a too burning sun. This is because extreme heat is an irritant; and, when consulted by consumptive patients, I never fail to give this injunction: Avoid cold, and *extreme heat*. Great heat stimulates the bronchi; simple catarrhal patients suffer from it like those that are consumptive, and tell you that an evening spent in too hot an apartment occasions them bad nights, that is to say, gives rise to coughs, oppression, fever, &c. A summer catarrh has recently been described; all practitioners have observed it; it is common among nervous people, *with delicate chests;* but it does not resemble the winter catarrh—it

is less humid; it is, in general, characterized by a dry *nervous* cough; the oppression, the quickness of the pulse, indicate irritation and congestion, rather than inflammation of the air-passages.

But if, instead of braving the sun, of seeking it, even, as a means of relief, patients mistrust it, keep in the shade without moving about, in an apartment protected against the invasion of external heat—if they choose the early and late hours of the sun's course to take their exercise—they then reap all the benefits of the warm season, without experiencing its inconveniences.

Well! apply these precepts to life in the colonies; impose upon patients the indolent habits of the Creoles of the Antilles, or of the Nabobs of India, and you will be obliged to deduct much from your lists of mortality. Instead of that, what have you done? You have taken your subjects of observation, and of statistics, from among the poor soldiers, or unfortunate sailors obliged to keep guard, or to work hard, under a vertical sun, and given the results as proofs of the pernicious influence of hot countries upon phthisical patients.

I may add, that navigators have pointed out the rarity of diseases of the chest within the tropics, which is also a legitimate corroboration of my proposition.

It is, therefore, not by the weakness they produce, that hot climates favour phthisis; it is, on the contrary, by the stimulation which extreme heat exercises on the respiratory passages when chronically affected. And in this, these climates are neither more nor less pernicious than our European summers; and if you come to an unfavourable conclusion with respect to the first, you must come to a similar conclusion with respect to the latter,—that is to say, you must announce an absurd proposition, against which the patients themselves will be the first to protest.

*Conclusion.*—Hot climates are unfavourable to phthisical patients only through the excess of solar heat. But by moderating this excess by obvious means, we obtain from a sojourn in hot climates the most happy results, confirmed by the testimony of numerous patients, who owe to emigration the prolongation of life, and occasionally perfect cure. It is this which renders residence in the neighbourhood of the tropics more favourable than under the equator, and which makes it better to live at Madeira, or in Northern Africa, than in Bengal or Brazil. And the reason why residence in the South of France is little favourable to phthisical patients is, that winter there still makes its rigour felt, against which the inhabitants neglect too much to fortify themselves.—*Gazette Hebdomadaire de Médecine et de Chirurgie*, June 8, 1860, p. 376.

*On the Diagnosis of Diseases of the Heart.* By Professor Skoda. Reported by B. Abelles.

*Pericardial Murmurs* are distinguished from endocardial: 1, by the character of slight contact and crepitus, while endocardial murmurs are blowing, grating, or moaning; 2, by their non-coincidence with the systole or diastole (drawling of the murmur); and 3, by the fact that these murmurs are heard only in a limited space, and not unfrequently vary with the position of the patient. *Endocardial murmurs* exhibit exactly the reverse; and their distinctness *not unfrequently* depends on the rapidity of the contractions of the heart, those which are caused by constriction of the ostium venosum becoming plainer when the heart's action is slower, while those produced by insufficiency become more distinct when it is more rapid. The diagnostic interpretation of murmurs arising within the heart will be possible only by considering the results due to a decided valvular lesion. Both insufficiency and constriction will, on account of the reflux of blood into the auricle, be attended with a diminished flow of blood into the aorta, and therefore with retardation of the circulation. The increased pressure of blood will overcome the resistance caused by the contractility and elasticity of the walls of the auricle, and so effect the dilatation of the latter, which will extend from it to its veins, and thence to the corresponding arteries, and finally even to the ventricle whence the artery arises. The permanent overfilling of these parts gradually leads, by increasing the labour of the ventricle, to hypertrophy of the auricle and ventricle, while in the parts of the heart not affected by the valvular lesion the reverse is the case. In insufficiency or constriction of the left ostium venosum, the circulation will consequently be feeble in the aortic system, while the hypertrophic right ventricle overfills the pulmonary vessels. The two factors, retardation of the circulation and the abnormal pressure in particular organs, will act injuriously on the nutrition of the system at large; and the cyanosis and dropsical effusions of patients labouring under heart disease are to be attributed not so much to the predominatingly venous or hydremic quality of the whole mass of the blood, as to congestion of the venous circulation; hence, too, may almost all disturbances of the functions of particular organs be explained, which so very much promote the sinking of the patient. Thus the various pulmonary affections are explained by overfilling of the pulmonary circulation; congestions of the venæ portæ and cava lead to intestinal catarrh, constipation, enlargements of the hemorrhoidal veins and granulation of the liver, congestion of the peritoneal veins to ascites. The same is true of the occurrence of Morbus Brightii and of the several abnormal phenomena connected with the nervous system. With reference to the distinctness of murmurs, the principle obtains that the murmur in general originates where it is most loudly heard. The question whether a murmur belongs

to the systole or diastole, is decided by ascertaining whether it corresponds in time with the pulse of an artery near the heart. Sounds and murmurs which coincide with the pulse or somewhat precede it belong to the systole, while those following the pulse are due to the diastole. The systolic heart-sound is best heard at the apex, the diastolic, on the contrary, somewhat higher and to the left, which phenomenon depends on the change of situation of the heart in passing from the systole to the diastole. The sounds of the right ventricle are heard in the middle of the lower half of the sternum; those of the pulmonary arteries in the third intercostal space on the left side, half or a full inch from the edge of the sternum; the sounds of the aorta are most audible at the insertion of the third left or right rib. Systolic murmurs indicate insufficiency—diastolic, constriction of the orifices of the heart, while in the aorta and pulmonary artery the opposite obtains.—*Vierteljahrschrift für die praktische Heilkunde*, 1861, Band 69. Analekten, p. 43.

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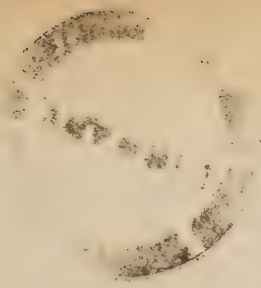
*On Hematuria.* By Professor Oppolzer.

In hematuria it is often difficult to determine whether the blood comes from the bladder or the kidney, inasmuch as, for example, we cannot with certainty infer the existence of renal disease from the presence of exudation-cylinders which microscopically exhibit the form of Bellini's tubes, for ordinary blood-coagula may accidentally present this form; therefore renal disease is to be assumed to be the cause of the hemorrhage only when the exudation-cylinders contain but few blood-corpuscles on their surface. The rapidity of the deposition of the sediment in the urine is by no means decisive in favour of vesical hemorrhage, as the duration of the contact of the urine with the blood is not known. The author considers the full investigation of the urine alone to approximate to a solution of the question. This in renal diseases exhibits decrease of the urea, uric acid, and colouring matters (particularly of Helier's urophaêin), increase of the urinary indigo, and an almost constant accession of albumen. If, on the other hand, symptoms of a vesical affection are observed, there is the possibility of vesical hemorrhage, especially if the urine comes in a decomposed state from the bladder. The causes of vesical hemorrhages are manifold: stagnation of the urine, spasm of the bladder, paralysis, injuries of the vesical region and of the bladder by foreign bodies (calculi), cancer; but certain medicines (cantharides) and changes of the system at large (scurvy) may also give rise to bloody urine. The same thing, moreover, happens spontaneously, as in the inhabitants of the Isles of France and Bourbon, or it is, as in Egypt, also caused by the hematobium, which may also give the impulse to the formation of urinary calculi. In all these cases the seat of the hemorrhage must be deduced from the symptomatology of the anomaly in question. Renal hemorrhages occur in Morbus

Brightii; and inflammations of the kidney are connected with renal calculi, cancer, and tubercle of the kidney, but are observed also in cardiac diseases, compression and obturation of the renal veins, in scurvy, and strongylus gigas. The treatment of hematuria consists in the treatment of the disease on which it depends.—*Vierteljahrschrift für die praktische Heilkunde*, 1861, Band 69. Analekten, p. 62.



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